1410

THE PHILOSOPHICAL

HISTORY

AND

MEMOIRS

OF THE

Royal Academy of Sciences at *Paris*:

OR,

An ABRIDGMENT of all the PAPERS relating to *Natural Philoſophy*, which had been publiſh'd by the *Members* of that *Illuſtrious Society*, from the Year 1699 to 1720.

With many Curious OBSERVATIONS relating to the Natural Hiſtory and Anatomy of Animals, &c.

Illuſtrated with COPPER-PLATES.

The Whole Tranſlated and Abridged,

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ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1714.

I. *Observations on the rain-water, the thermometer, and barometer, during the year* 1713 *at the royal observatory, by M.* de la Hire*.

THERE fell in

	Lin.			Lin.	
Jan.	19		July	60	$\frac{2}{8}$
Feb.	12	$\frac{2}{8}$	Aug.	24	$\frac{7}{8}$
March	8	$\frac{5}{8}$	Sept.	16	$\frac{5}{8}$
April	29		Oct.	17	$\frac{7}{8}$
May	25	$\frac{2}{8}$	Novem.	8	$\frac{3}{8}$
June	22	$\frac{5}{8}$	Dec.	2	$\frac{2}{8}$

Sum of the water of the whole year 247 lines $\frac{1}{2}$, or 20 inches, 7 lines $\frac{1}{2}$.

We see by this, that the quantity of water which fell this year, is a little greater than in the mean state, which we have settled at 19 inches. We see also that the single month of *July* has afforded almost a quarter of it, and it usually happens, that in the 3 months of *June*, *July*, and *August*, there falls as much as in all the rest of the year. It rained a good deal in *April*, but in *March* it rained but very little, for the wind in

* Jan. 10, 1714.

this month was generally north, or thereabouts. The ſame alſo happened in *November* and *December*.

The greateſt cold of this year was *January* 15, when my thermometer fell to 18 ½ parts, which is not a very conſiderable cold, for in ſeveral years we ſee it fall to 14, and it is at 32 when it begins to freeze in the country; the winter however was cold, and it continued freezing during the whole month of *March*. It was pretty cold alſo during the laſt two months of the year, and it often froze; and in the ſtrongeſt cold of theſe two months the thermometer fell to 22 ½ parts *December* 6. There fell no ſnow during the whole year.

The heats were not great, for the thermometer roſe but very little above 48, which marks the mean ſtate of the heat of the air, as it is always at the bottom of the caves of the obſervatory; but only the laſt days of the month of *Auguſt* the thermometer roſe to 58 parts. It may be obſerved, that in ſummer the thermometer uſually riſes 11 parts about two or three hours after noon higher than in the morning at ſun-riſing, which is the time when I make all the obſervations.

There were a great many fogs toward the end of this year, and the little heat of the month of *Auguſt*, did not give a perfect maturity to the fruits.

The prevailing wind of this year was N and E. wherefore the heats were not great, and did not laſt long. The great rains of the month of *July* came with winds from the ſouth or thereabouts, which ſhould have brought a hot air, only they were cooled by the weſt wind which comes to us from the ſea, and by a great quantity of water upon the ground; for the water cannot receive the impreſſion of the heat of the ſun ſo ſtrongly as a

dry

dry ground; the cold and moiſt winds are alſo more penetrating than the dry; and if we ſometimes feel a ſuffocating heat in ſummer after a ſmall rain, it is only becauſe this rain partly abates the wind which uſually comes from the ſouth when it rains, and becauſe the impreſſion of heat that we feel is much greater when there is no wind, than when any wind whatſoever continually carries off a ſort of heated atmoſphere, which ſurrounds our bodies.

My barometer was at the higheſt at 28 inches 4 lines $\frac{1}{2}$ *November* 26, with a pretty weak north-eaſt wind, and the days before and after, the wind was the ſame, and in all that time it did not rain, for in the 2 months of *November* and *December* there was hardly any rain, and the barometer always kept up very high. *October* 29 the barometer was at the loweſt at 26 inches 10 lines $\frac{1}{3}$, the wind being at that time towards the ſouth but with little rain.

In *July* when it rained in two days near 25 lines, the barometer was at 27 inches 3 or 4 lines. We ſee therefore that the difference between the greateſt and leaſt height of the barometer, was this year 1 inch 6 lines very nearly, which is as uſual. I have another barometer ſituated in the ſame place, where the quickſilver is always 3 lines higher than in that which I uſe for my obſervations.

We may conclude from all this, that in general, when the air is heavier than in its mean ſtate, it rains but ſeldom and very little, and on the contrary when it is lighter there uſually falls ſome rain, as has been always obſerved ever ſince the barometer began to be uſed. However there are ſometimes found in the air diſpoſitions of cold or heat at certain diſtances from the earth with low winds and fogs, which cauſe rain when we ſhould judge

from the barometer that it would be fair; for the weight of the air is sometimes not great enough to raise the vapours which form the fogs, or else it raises them but to a little height, and so they fall again almost immediately in rain after being condensed; it is common also for it to rain soon after the fog is risen.

December 29, I found the declination of the needle to be 11 degrees 12 minutes toward the west.

II. *Observations on the gum-lacc, and other animal substances, which furnish the purple dye; by* M. Geoffroy *junior.*

Of all the curiosities of natural history none have caused greater admiration than the labours of the bees; and tho' these wonderful insects greatly merit our attention, yet they are not the only ones which make their industry appear in building hives. Many others have the same art, but the little benefit that we receive from the labour of some of these animals has occasioned their being neglected.

Among these we may reckon the insects which produce the *gum-lacc* in the *Indies*. And tho' this material has been sought after for dying and other uses, yet the manner in which it is produced has not been discovered, either for want of observers upon the spot, or because the insects which work it not being brought up with particular care, like our bees, have not been closely enough examined.

I shall therefore add my observations on this subject, to the little information which authors have given us concerning it, that we may shew the nature of the gum lacc better than has been hitherto done.

The

The name of gum ſeems not very proper, for it is rather a ſort of wax, as I ſhall ſhew preſently.

The name of *lac* or *loc* comes from the *Arabians*, who taught it to the *Indians*; it is alſo called *trec* in the kingdom of *Pegu* and *Martaban*.

The firſt who have treated of the lacc have informed us that it grows in the *Indies*, and particularly in the kingdom of *Pegu*.

As it is difficult for us to believe that the ancients were ignorant of any thing, ſeveral have imagined that it was known by *Dioſcorides* and *Serapio*, and that it is what the firſt has called *cancamum*; but the deſcription which theſe authors have left of it, is too imperfect for us to be able to make any judgment of it.

The principal ſort of lacc, and that which has given occaſion to my obſervations, is called *ſtick-lacc*, becauſe it is brought to us faſtened upon little branches on which it has been formed.

The firſt thing to be examined, is whether this gum does not proceed from theſe little branches to which it is faſtened. This is not very probable, becauſe in breaking and looſening it from theſe little ſticks, we find no outlet, through which it could flow. Beſides as this gum is in great abundance, and as the ſticks are often very ſmall, it is viſible that it is not produced from them: this is alſo F. *Tachard*'s opinion, who relates, that when an inciſion is made in theſe ſorts of trees, there comes a gum from them, but that it is of a quite different nature from the lacc.

We know in general it is the work of a ſort of inſect; ſome ſay they are flying ants, that depoſit this matter upon the ſlender branches of a tree called *Ber*. Others will have it that they are common ants, and others that they are flies; ſome even ſay that the ſticks of the lacc are nothing but branches,

branches, which the inhabitants take care to prick into the ground in a large quantity, to ſerve for a ſupport to the work of theſe little inſects.

As for the nature of this work, they have not given us any certain information.

It has appeared to me upon a careful examination that it could be nothing but a ſort of hive approaching in ſome meaſure to that which bees and other inſects are accuſtomed to work. And indeed upon breaking it, we find it divided into cells of a pretty uniform figure, which ſhews that it has never been a gum or reſin flowing from the trees.

Each of theſe cells is oblong, and has ſeveral ſides, ſometimes quite round, according as the matter while ſoft has been diſordered, and flowed about the branch which ſuſtains it.

This diſordering is the cauſe that the cells have ſome difference in their conſtruction, however they are for the moſt part ſwoln in the middle, ending in a ſharp point on that ſide where they touch the branch, terminated by the other in a point, more or leſs rounded, and perforated with a little hole, ſuch as thoſe with which the whole ſurface of the lacc appears to be perforated when viewed more cloſely.

At the extremities of the cells there are uſually perceived two little white lines, of which it is very difficult to determine either the nature or uſe.

The partitions of theſe cells are extremely fine and perfectly like thoſe of the bees; but as they have nothing to defend them from the injuries of the air, they are covered again with a layer of the ſame wax as hard and as thick, to ſerve them for a ſhelter.

Hence we may conjecture, that theſe animals work with no leſs induſtry than the bees, ſince they have much fewer conveniences.

Theſe

Theſe cells are certainly made to lodge ſomething, and it is not a mere excrement which theſe inſects depoſit, as ſome have imagined; for there are diſcovered in them little bodies more or leſs ſwoln and moulded therein. The firſt obſervers took them for the wings or other parts of theſe ants to which they aſcribe the lacc, in which they thought them to be incloſed. Theſe little bodies are of a fine red, ſome deeper and others paler; and when cruſhed they are reduced to a powder of as fine a colour as that of the cochineal.

It is not probable, that the parts of theſe animals form the lacc; at leaſt they are not ſufficiently diſtinguiſhed: to what purpoſe ſhould they make ſuch a tomb for themſelves?

There is more reaſon to believe, that theſe cells are intended for their ſwarms like thoſe of the bees, and that theſe little bodies are the embrio's of the inſects, which are to come out, or the coverings of thoſe which are actually gone out; as we ſee in the kermes or ſcarlet grain, the galls and other excreſcences, which proceed from the punctures of inſects, of which I ſhall have occaſion to ſpeak, for the farther clearing up of this matter.

Theſe little bodies are oblong, wrinkled or ſhagreened like the cochineal, terminated on one ſide by a point, and on the other by two, and ſometimes by a third. When ſoaked in water they ſwell like the cochineal, give it as fine a tincture, and aſſume almoſt the ſame ſhape, ſo that by mere inſpection we judge them to be the little bodies of inſects in whatſoever ſtate they may be. The lacc owes its red tincture to them, for when it is quite deprived of them, or but little furniſhed, it hardly affords even a ſlight tincture.

It appears therefore that the lacc is only a ſort of wax, which forms in a manner the body of the hive.

This

This wax is of a good ſmell when burnt, but as for the little bodies incloſed in the cells, they yield a diſagreeable ſmell, like the parts of animals.

Many of theſe little bodies, which for the moſt part are hollow, are ſometimes found quite rotten, and full of mouldineſs; others are filled with a powder, in which the microſcope diſcovers a great number of long tranſparent inſects, with ſeveral feet, of which it is not eaſy to determine the ſpecies. Perhaps they are the inſects which have been there depoſited by thoſe which form the lacc; or perhaps they may be others, which are there produced.

For there are different ſorts of them, which penetrate the whole ſubſtance of the lacc, lodging therein, and depoſiting their eggs in great number, as it happens ſometimes in the bee-hives: this is eaſy to be obſerved by a ſilk which paſſes on the outſide, through an aperture which the worm makes as it goes in, and under which we diſcover its covering, which is called *Nympha*, *Chryſalis*, or *Aurelia*, and is commonly pretty thick.

Near this covering we ſee a knot of round, flat, tranſparent eggs, and as red when cruſhed, as the little bodies contained in the cells.

This ſilk, of which the end appears on the outſide, ſerves to cover the *chryſalis*, and ſometimes for a ſupport to the eggs.

The worm could never have lodged itſelf thus without deſtroying a great number of cells, and finiſhing that which it found; it is to this chiefly that it faſtens itſelf, doing no damage either to the wood which bears the lacc, or to the upper layer of the ſame lacc, where we only obſerve a pretty large

large hole, by which it went in, and came out after its metamorphosis.

All these observations lead me to conclude, that what is called gum lacc is not exactly of the kind of gums or resins, but a sort of wax collected and formed by certain insects, after the manner of hives, of which the cells are filled with their swarm; and that the white stripes, which I observed in the cells, may be a remainder of a matter, fit to nourish these little insects, when they begin to hatch.

But what confirms me more in this opinion, that the lacc is a real hive, is the examination I have made of that which comes from the island of *Madagascar*, for it does not differ at all from wax, having the colour and the smell of it. The natives call it *Lit-in-bitsic*. *Flacourt*, in his description of the island of *Madagascar*, is the first who has made mention of it, the pieces of it are much thicker than those of the ordinary lacc; its colour approaches to that of the transparent white amber; it is formed like that of the *Indies*, about the branches of trees, and is in like manner disposed in cells filled with a larger sort of *chrysalides*, the figure of which answers to that of the common lacc, but the colour of it is grey.

These membranes might rather be taken for wings of ants, because of the colour, whereas the membranes or *chrysalides* of our lacc are so red as to appear black; besides this lacc of *Madagascar* is not used like that of *Pegu*, being of no use in tinctures or in sealing wax, which makes it also be less known.

By all the experiments which I have made on this lacc, I have not found it different from the wax of our bee-hives, which might give room to suspect, that this matter might all be the work

of ſome ſorts of bees, if the figure and diſpoſition of the cells did but viſibly appear the ſame as in the lacc. The ſpecies of *chryſalides* have all the ſame figure, except the colour, and the upper and lower part of the cells have the white ſtripes, which I have obſerved in our lacc.

The conformity, which I obſerve between theſe two different laccs, muſt take away all ſuſpicions about the ſtick lacc; ſo that it is out of doubt that theſe two ſubſtances are not produced by the trees on which they are found, but that they are brought from ſome other place by inſects, which according to the account that F. *Tachard* received, are a ſort of ants, that gather it as our bees do wax. Now it is known that our bees do not make the wax, but only gather it from flowers, as M. *Maraldi* has juſtly obſerved, ſo that without the bees we ſhould have no wax; and though we know where they find it, yet all the induſtry of men imaginable could never gather, with much labour and in a long ſpace of time, ſuch a ſmall quantity as is the common load of one bee.

I may therefore compare the lacc upon the ſticks loaded with cells to the wax of our bees, and ſay, that without the ants there would be no lacc, for it is they that take the pains to gather it, prepare it, and work it, during 8 months of the year, for their particular uſe, which is the production and preſervation of their young, and that men have taken advantage of it by uſing it for the fine ſcarlet tincture made in the *Levant*, and for ſealing wax and verniſh.

I do not however pretend entirely to overthrow the opinion of thoſe, who ſay there is a gum-lacc, which diſtills from the leaves of certain trees, ſince the ants, which according to F. *Tachard*'s account, uſually gather it upon different flowers, might find

find it alſo in abundance upon ſome trees, whence it might be gathered without their aſſiſtance. Thoſe who are of this opinion think it is the lacc that is brought to us in maſs, in which we find neither the cells nor the little red bodies that are ſeen in the other. But it is much more probable, that all this matter has been wrought by the ants, and that the maſſes were thus brought by the *Indians*, after having drawn the tincture from it, or cleanſed it from the impurities with which it was mixed. Theſe alterations are not without example, for we ſee that the workmen who make uſe of the ſtick lacc to draw the tincture from it, metamorphoſe it into another ſort of lacc, called ſeed lacc, becauſe it is like little ſeeds or grains, and may be uſed for other works, for which it is even preferred to that which is in maſs.

This is what we may conjecture concerning the lacc in maſs, for the facts muſt be verified upon the ſpot, unleſs we chooſe rather to depend upon *Tavernier*, who ſays, that in *Pegu* there is a leſs eſteemed ſort of lacc, which is commonly mixed with a great deal of droſs, and the reaſon which he gives for it is, that the ants lay it in the ground in heaps, ſometimes, ſays he, as big as a tun.

We muſt therefore eſtabliſh different ſorts of lacc; firſt the lacc in branches, of which I diſtinguiſh two ſorts, one of a deep yellow amber colour which has the cells, but has not received heat enough to take the red colour of the animal parts contained in it, or rather becauſe the wax is in too great abundance in proportion to the animal parts contained in them.

The 2d ſort is of a darker colour on the outſide, but quite red when held up againſt the light; this fine colour comes from the cells being well filled, and the animal parts being in abundance, and com-

municating their tincture to the wax with the assistance of the heat of the sun; we may say it is the lacc in its maturity, for it is more heavy, close, and solid than the preceding.

The 3d sort is a dirty lacc in bits, mixt with earth, and wood, wherein we can hardly discover any cells; this sort may be that which *Tavernier* means.

The 4th sort is that which I have described from the island of *Madagascar*, which, as I have observed, comes much nearer our wax than the common lacc.

The 5th sort is that which is in loose grains of a reddish colour.

The 6th is in a great mass or loaves, resembling a heap of little grains united together, and of a brown colour; this last species would agree well enough with that which Mr. *Ray* has mentioned, if it is true, as he supposes, that it flows naturally from trees.

Having shewn that the lacc in branches is nothing but a hive wrought and formed with as much care as the combs of our bees, and for the same use, the production of some new swarm of these insects; I shall go on to the other uses, which have been accustomed to be made of these substances.

The principal part of the lacc is, as I have shewn, what is inclosed in the cells, and has been called by me a sort of *chrysalis*.

Tho' it is probably the least considerable, and indeed the least observed, yet it is from this that the lacc derives the colour which renders it remarkable, and recommended itself to the first who thought of making that fine tincture of it, which has always been most esteemed by all people; this species of *chrysalis* taken from the cells, and in-

fused

fuſed in water or ſpirit of wine, gives theſe two liquors as fine a red colour as the cochineal, which is at preſent uſed for the fineſt ſcarlet dyes.

The matter of the cells on the contrary cannot communicate it to the different liquors, but in proportion to what it has received from the *chryſalides*, as has already been ſaid.

It is a proof of what I advance, that the cells of the firſt ſpecies of lacc, which have hardly any colour, yield but very little, or none at all, in different trials.

The moſt eſteemed lacc for tinctures is that which is in branches, becauſe it is more furniſhed with animal parts. The moſt red is commonly choſen.

According to *Tavernier*'s account, that of the kingdom of *Bengal* is dearer upon the ſpot than that which comes from the kingdom of *Pegu*, becauſe the inhabitants of *Bengal* make uſe of it for the fine ſcarlet colour which they give to their linnen: it is alſo carried into *Perſia* for the ſame purpoſes.

He informs us alſo, that the lacc of *Pegu* is the laſt eſteemed, becauſe in that country the ants are the moſt negligent, for they depoſit it in heaps on the ground.

To obtain the red tincture of the lacc, accord-to F. *Tachard*'s account, it is ſeparated from the branches, and beaten in a mortar, and throw it into boiling water; and when the water is well tinged, they throw on ſome more till it tinges no longer. They evaporate the greateſt part of the water in the ſun. They afterwards put this thick tincture in a clean linnen cloth, bring it near the fire, and ſqueeze it through the cloth. That which paſſes through firſt is in tranſparent drops, and

and is the fineſt lacc. That which comes through afterwards by a ſtronger expreſſion, and which they are obliged to ſcrape with a knife, is browner, and of leſs value. This is the moſt ſimple preparation of the lacc, which is only an extract of the red colour afforded by the animal parts.

This matter thus extracted preſerves the name of lacc, and it is from this firſt preparation that the reſt, which have been ſince introduced by the aſſiſtance of art, take their name.

Thence come all the laccs or lakes uſed in painting, which are dried paſtes, to which the colour of the lacc has been given according to the different degrees neceſſary for the gradation of colours. From this word there has been extended a great number of dry paſtes, or powders of different colours, and tinged with very different materials.

We ſee alſo, that of all the red powders now in uſe, we have only the lacc of the cochineal, which has changed its name to that of *carmine*, which is not its true name, being borrowed from the *kermes*.

There is a more compound preparation of lacc made to dye the *Morocco* leathers.

It is probable that the fineſt *Morocco* leathers of the *Levant* are prepared with this material, tho' we are not as yet informed of it by any relation. This is certain, that thoſe who prepare them in *France*, and have imitated the manufacture of the *Levant* pretty well, make uſe of the lacc; whether they have been led to it by gueſs, or by any ſecret knowledge they have had of it, for they have made a ſecret of it. I ſhall mention what I have been able to diſcover concerning it.

They chooſe the ſtick lacc of the higheſt colour, they ſeparate the ſticks from it, reduce it

to

to powder, and throw it into boiling water with ſome prickly and light galls, ſome allum, and a little cochineal. When the tincture is made, they dip the goat-ſkins in it, after having prepared them in a particular manner, which they call the *confis*. It is dipping the ſkins to curry them in a bath made with dog's dung, becauſe theſe excrements are probably more proper to exalt the tincture, than the tan or other like drugs. Theſe ſkins being well waſhed and ſewed double, that the colour may take but on one ſide, are ſteeped in this tincture till they are ſufficiently coloured, after which they are dried, and made gloſſy with linſeed oyl.

They treat the red ſheep-ſkins, which are manufactured at *Limoges*, after a different manner. They uſe nothing to ſtain them, but a common lacc drawn from the *Brazil* wood, without any of the ſtick-lacc.

The matter which remains after the tincture for the *Morocco* leathers, is in light, bloated lumps, of different figures, of a red colour, more or leſs deep, which the workmen call *gravois*. They are uſed with other materials for the common *Spaniſh* wax.

The preparation, which is made of that part of the lacc, which forms the cells, ſerves to manufacture the flat lacc, which is a lacc melted and caſt into broad, thin, tranſparent leaves. Some of it is in little flat bits, round on one ſide, and hollow on the other, which is called *laque á oreille*, or *ear lacc*, becauſe of its ſhape.

All theſe laccs are uſed in varniſhing, but eſpecially the ſtick lacc, and the flat lacc.

The

The analyſis of the lacc ſerves alſo to confirm the compariſon that I have made of it with the wax of our hives, for we obtain much the ſame principles from it, an acid ſpirit, and a butter, as M. *Lemery* had obſerved. But as the animal parts contained in the lacc muſt furniſh ſomething particular, and as it is not eaſy to analy ſize them ſeparately, I have found myſelf obliged, in order to obtain my end, to take a ſort of lacc, which being reduced into grains after having ſerved for tinctures, is abſolutely deprived of all the animal parts that it can contain; and I have obſerved by the analyſis between this ſeed lacc, and the ſtick lacc, a difference, which juſtifies my conjectures concerning the little animals incloſed in the cells; for the acid ſpirit obtained from the laſt is mixed with a volatile ſpirit, which only the animal part can furniſh, as may be eaſily known from the white precipitate, which reſults from its mixture with the ſolution of the corroſive ſublimate, which the acid ſpirit drawn from the ſeed-lacc does not, becauſe it contains none of theſe animal parts.

In obſerving the animal parts contained in the two ſorts of lacc, I have been obliged to compare them with the other animal ſubſtances from which the purple tincture is drawn. One of theſe, which has been long in uſe, is the kermes, which I may rank among the galls, becauſe it is an excreſcence produced upon a tree by the puncture of an inſect. *Kermes* is an *Arabic* word, which ſignifies *worm*, and tho' we cannot queſtion but that the kermes was known by the antients, and in uſe among them; yet we know confuſedly, that a number of different ſubſtances, which grow in different countries, and upon ſeveral ſorts of trees and plants, have alſo ſerved for ſcarlet tinctures; but of

of all theſe ſubſtances the kermes is the only one that has hitherto been employed in our dyes. They are little thick bladders, as round as peas, faſtened upon the *ilex aculeata cocci glandifera*, C. B. P. 425. There is obſerved in the ſpring upon the leaves, and young ſhoots of this ſhrub, a little bladder, which at firſt is no bigger than a millet ſeed; it is cauſed by the puncture of an inſect, which there depoſits its eggs, it grows inſenſibly, and becomes of an aſh-colour on its ſurface; this colour is nothing but a ſort of meal like that upon fruits, which hides their red colour. The *Greeks* have called this grain *coccos baphica*; the *Arabians*, *kermes alkermes*; the *Latins*, *coccus* or *vermiculus*, whence we derive the name of *vermillion*. That of *ſcarlet*, which is the moſt in uſe, is thought to come from the antient *Celtic*. When this gall is perfectly ripe, they gather it, and draw out the juice or pulp, by cruſhing it, or elſe ſprinkle it with vinegar to kill the inſects that are incloſed within. Without this precaution, theſe little animals coming to hatch, would leave the ſhells empty. They come out like little flies, ſuch as drown themſelves in vinegar, and other odorous liquors expoſed to the air. After having examined theſe ſhells when dry, I obſerved a great many of them to be empty, and that thoſe had a hole at the place where they adhered to the leaf of the tree. This ſort of gall is very light, pretty brittle, and of a lively ſhining red; it is covered with a very thin, brittle, membranous pellicle, excepting the place where it holds to the leaf; for on this ſide it is a little flatted, and we perceive a round, white, fleſhy matter, which ſerves as a pedicle to the berry, and at the ſame time as a cover to hinder the air from getting in. The ſkin of the berry makes at the juncture of

the pedicle a ſort of roll, which is let into the pedicle: below this firſt ſhell there is another more tender, tho' thicker, of a red colour, which is probably the juice dried, and the pulp which ſuſtained the inſects contained therein.

This ſecond ſhell contains 2 or 3 cavities, which vary like thoſe of certain fruits or berries. The largeſt is filled with a duſt like a worm-hole. There may eaſily be 2 colours obſerved in it, the white, and the red, which is commonly called *paſtel d'ecarlatte*, and is uſed by the dyers.

Upon viewing this powder with the microſcope, the white parts ſeem to be ſhaped like very ſlender horns, which in all probability have ſerved for coverings to the inſects.

In order to obtain the better knowledge of the principles of the *kermes*, I have made the analyſis of it. Beſides the phlegm, which had the ſmell of the kermes, but anſwered to no trials, I have obtained ſeveral portions of urinous and volatile liquors, which have not altered the tincture of the tourneſol, but have turned that of violets and roſes green.

It is remarkable, that from a pound of kermes, which I put into a retort, I obtained half an ounce of a fine cryſtalliſed volatile ſalt, and the value of a dram or 2 mixed with a yellow oil. The fetid oil was in great abundance without being black; it was of a deep yellow, and as thick as butter. I cannot compare its principles better than to thoſe of the ſilk, which was analyſed by the late M. *Tournefort*.

The kermes therefore is a ſubſtance, which has more of the animal, than of the vegetable, or rather which has been entirely transformed by the inſect into an animal ſubſtance, as the analyſis demonſtrates,

monſtrates, ſince it furniſhes a very great quantity of volatile ſalt, and of very pure ſulphur.

As for the uſe of the kermes in dying, it is not very conſiderable, and without that in medicine, the gathering of it would perhaps be neglected, and we might perhaps know it only upon the relation of thoſe who had ſeen it gathered in *Languedoc*, as we have loſt the uſe of other ſhells or worms, which were formerly uſed, which we know at preſent only by the account of learned men who have obſerved them. This is what has happened to ſeveral other animal ſubſtances, which ſerved alſo for the purple tincture, ſuch as the purple of the antients, which M. *de Reaumur* has obſerved and deſcribed, as alſo the inſects of the root of burnet, thoſe of the maſtick-tree, pellitory, plantain, and knawel, which are found in great quantity in *Poland*, and ſeem to deſerve ſome attention.

This material is a little more known, becauſe it has been obſerved again in our time. Some call it *Poliſh cochineal*; it is a ſhell found at the root of this plant, which is a ſort of *Alchimilla gramineo folio*. The peaſants gather it in *June*; in this ſhell there is found a red worm, the blood of which yields a fine tincture, which the inhabitants have often found by the red excrements of the fowls which had eaten them.

Theſe inſects were formerly carefully gathered, and drowned in cold water, or in oil.

The *German ephemerides* ſay, the *Dutch* uſe them ſtill in dying, together with the cochineal, and that they gather them without injuring the plants. It is done with a ſort of hollow trowel made on purpoſe for this uſe, but this inſect is no longer known by the dyers.

The *cochineal*, for the beauty of its colour, and the great quantity of it, which comes to us from *America*, has in a manner made uſeleſs all the other materials, which were formerly uſed.

Tho' it is very common, yet the authors, who have treated of it, are divided about it. Some have ſaid it was an inſect, and others, on the contrary have maintained that it was a grain.

This diverſity of opinions is obſerved in the firſt authors who have treated of it. It is true, that we have had in the laſt place the teſtimony of F. *Plumier*, a very skilful botaniſt, who was of great weight in perſuading us, that the cochineal, which is called *Meſteque*, is nothing but a little animal like a bug, which is gathered upon an *American* plant called *opuntia major validiſſimis aculeis munita*, Inſt. R. H. or the *Indian-fig*; but on the other hand, *Pomet*, in his hiſtory of drugs, pretended that this father was miſtaken, and quoted the letters of a friend of his, actually on the ſpot, who affirmed, that the cochineal was a ſeed.

Tho' this opinion has not been much followed, yet I do not know any body, who has refuted it expreſsly, and removed the doubt which this contrariety might leave.

One would think the meer inſpection of the cochineal would be ſufficient to terminate the difference, but in the dried condition in which it is brought to us, we diſcover nothing to make us lean either one way or the other.

Having a mind therefore to be better informed, I put ſome in water, where it ſwelled and acquired a ſuppleneſs, and a form, which can agree only with an inſect. I could plainly diſcover the head, *anus*, rings and the places of the legs without the help of a microſcope. I have prepared ſome in ſuch a manner that the legs might be ſeen articulated,

lated, 3 on a ſide; but I have not perceived any ſigns of wings, as ſome have pretended, who would have the cochineal to be a ſort of beetle. Upon opening ſome of the biggeſt, there came out a red liquor, with a ſort of bunch faſtened to a hollow canal, furniſhed with little grains, which being viewed through a microſcope, looked like little cochineal.

This configuration both internal and external of the parts of the cochineal, ſhews ſufficiently that it is really an inſect, as F. *Plumier* ſays, who has compared it juſtly enough to a bug; but however it is not ſo flat, and I believe, becauſe of its roundneſs, it is more like thoſe inſects, that are found upon dogs.

It is ſurpriſing, that the cochineal ſhould draw its fine red, from leaves that are of a pale green; for F. *Plumier* obſerves that theſe inſects grow upon different ſorts of trees, but that they never give this fine colour, except they are fed with the leaves of the *opuntia*. How are theſe leaves able to afford the colour which they have not themſelves? To reſolve this queſtion, we may obſerve, that the *Indian-fig*, which is the fruit of the *opuntia*, is of ſo ſtrong a red colour, that it even tinges the urine of thoſe who eat it, which has ſometimes affrighted thoſe who did not know the cauſe of it and thought they had voided pure blood; we may therefore conjecture, that the ſubſtance of this plant ſuffers, as it paſſes into the body of this little inſect, the ſame alteration, as that which cauſes the ſame colour in its fruit.

I have obſerved, that there faſtens upon the red roſes a little white worm, of the length of 4 or 5 lines, with a red head: its food, which is the roſe itſelf, almoſt loſes its colour in its ſtomach, which is eaſy to ſee, becauſe it is tranſparent, we perceive only

only a bluiſh colour in it; if you cruſh theſe worms upon paper, they leave a green colour, as muſt happen, becauſe the volatile parts, and alkaline juices of animals uſe to turn the juice of roſes green, but this juice receiving a new alteration, at the extremity of the inteſtine, is the cauſe, that the excrements of theſe worms, which are found in ſmall grains like gunpowder, have a red colour like roſes, and yield by means of ſpirit of wine aſſiſted by ſome acid liquors, the ſame tincture as roſes, and ſuffer all the ſame alterations, as turning green by alkalies, and red with acids.

If the obſervations now related were not ſufficient to prove that the cochineal is an inſect, the analyſis, which I have made, would alone ſufficiently demonſtrate it: for we draw from it exactly the ſame principles which the parts of animals uſe to furniſh; a phlegm of the ſame ſmell with that which comes in the diſtillation of harts-horn, a ſpirit and volatile ſalt much in the ſame proportion as is drawn from ſome inſects, as beetles, and a fetid oil of a deep red, and pretty thick.

Beſides the uſe which is made of the cochineal for all the fine tinctures, it is alſo employed for the carmine, which was formerly made with the *kermes*, and is at preſent prepared with cochineal, allum, **chouan*, and †*autour* boiled in water, they gather the red ſcum, which is the fine carmine, the reſt of the maſs, which has ſtill a good deal of colour, ſerves to make the lacc or lake of the painters; it ſerves alſo for the ſcarlet tinctures, which were drawn from the ſame kermes, but it is by exalting its colour, which is naturally purple, by means of

* A little ſeed of a greeniſh yellow colour, brought from the *Levant*.

† A bark ſomething like cinnamon, brought from the *Levant*.

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a diſſolution of fine pewter in *aqua fortis* regaliz'd with *ſal ammoniac.*

The pewter on this occaſion has a very different effect from that which it makes with the tincture or diſſolution of gold, ſince a drop of the ſolution of pewter thrown upon a certain quantity of ſolution of gold, gives it a fine purple colour, whereas here it extends the purple colour, which the cochineal naturally has, to make a very lively red.

We ſee by what knowledge we have of the materials, that have at all times been employed for this precious tincture, that they have always been taken from the animal kingdom. Thoſe, which are furniſhed by madder, ſafflower, brazil-wood, and other vegetable ſubſtances, have never come near it for beauty.

The firſt, which was in uſe among the ancients, was taken from a ſhell-fiſh *murex*: the *Tyrians* found it out by chance, if it is true, as the antient authors tell us, that a dog, having devoured one of theſe fiſhes on the ſea ſhoar, had his mouth ſtained with ſo beautiful a colour, that it cauſed admiration in all that ſaw it, and an earneſt deſire of uſing it.

This tincture, more precious than gold itſelf, tho' it was then ſo rare, continued long in uſe, 'till they diſcovered, by I know not what accident, the kermes, afterwards the lacc, and at laſt the cochineal, all of them animal ſubſtances, and the only ones that have been found, by the experience of ſo many ages, to be fit to give to precious ſtuffs their greateſt beauty.

III.

III. *A justification of the measures of the ancients with regard to geography; by M.* Delisle*.

Tho' the measures of countries, as they are related by the ancients, were very far from truth, it would always be very curious to know them; but I now undertake to prove, that this enquiry is very useful and necessary, and these measures being conformable to the truth, they must serve to correct in several essential points the measures of the modern geographers.

I shall establish the exactness of the ancient measures, by the conformity that they have with the observations of the academy. It will without doubt seem surprising, that the ancients have come so near the truth, and that the modern geographers on the contrary have been so far from it, but I hope to point out the source of the error of the one, and the ease that the others had to speak more justly of the situation and extent of the countries which they have described.

What I advance has no relation to those parts of the earth, which the *Romans* knew but imperfectly, but only the countries with which they were acquainted, as *Italy*.

To make the difference between the ancients and moderns concerning that country appear, I have here † made a double representation of it, in one of which, marked with faint lines, I represent *Italy* according to the opinion of most of our moderns; and in the other marked with stronger lines, I represent it according to the measures of the ancients, conformable, as I have said already, to astrono-

* April 11 1714. † Plate I, Fig. 1.

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mical obſervations, and particularly to thoſe of the academy.

To make the differences between the two opinions appear the plainer, I thought it neceſſary to chooſe a point, with regard to which we might eaſily compare the two *hypotheſes.* It was no difficulty to determine upon the choice of this point, and we cannot take a more favourable one than the city of *Rome* itſelf, whither they came from all the parts of the *Roman* empire, and from which they ſet out to all the provinces; for it was at *Rome*, that the fine military ways began, which the *Romans* had cauſed to be made with ſo much care and expence, and upon which they had marked their meaſures, which will facilitate the compariſon of them with the obſervations.

What has remained 'till now of theſe ancient ways, ſhews us that they were generally ſpeaking very ſtrait, as the *Appian* way, which ſtill remains almoſt entire, between *Rome* and *Capua*, tho' conſtructed above 300 years before the Chriſtian *æra.*

It croſſed the *Pontine* marſhes in a right line, which had been raiſed in this place; in other roads they penetrated through mountains, as we ſtill ſee in the place formerly called *Intercifa*, and now *Saſſo-forato*, where the *Flaminian* way croſſed the *Appenine* mountains.

This is a great point in favour of the ancients, for theſe ways being ſtrait are much fitter to determine the diſtances from one place to another, than the preſent. Beſides we know the *Romans* meaſured their roads, and raiſed ſtones at every mile, or thouſand paces; and as theſe paces were each of them 5 *Roman* feet, they had thereby an uniform meaſure through the whole extent of their dominions, whereas now the miles are of different lengths in the different parts of *Italy.*

This uniformity of the meaſures marked upon roads, which came very near to a right line, has not permitted me to waver in the preference which I have given to the ancients; but before we make uſe of theſe meaſures, we muſt be ſure of their value with regard to ſome known magnitude.

The moderns have ſuppoſed the ancient mile to be equal to a minute of latitude, as the common mile of *Italy* now is, 60 of which make a degree.

But having compared the diſtances ſet down by the ancients in miles, between ſeveral cities of which we know the modern name, with thoſe of the ſame cities determined in toiſes by the moſt exact geometrical methods, theſe compariſons joined with the meaſure of the earth by the academy, always give for each degree of latitude 75 of theſe ancient miles inſtead of 60, as I hope to ſhew more particularly, when I relate theſe geometrical operations.

It follows from thence, that the ancient mile was leſs than the modern, and that tho' even this laſt was uniform through all *Italy*, which it is not, the diſtances of the towns related by the ancients could not be the ſame as thoſe which are now authorized by cuſtom; and this has led moſt of our moderns into miſtakes, who not knowing ſufficiently the real magnitude of the ancient miles, have unluckily corrected an exact and uniform meaſure by a very unequal and uncertain one. Among others *Cluverius*, one of our moſt famous geographers, who has given us ſo ample and ſo particular a work on the ancient *Italy*, which *Holſtenius* and *Cellarius* have hardly been able to improve; *Cluverius* I ſay, has made no difficulty to take up the ancient diſtances in ſeveral places, and to regulate them by the modern.

The obſervations of the academy made at *Rome* and at *Florence*, have given us the true ſituation of

theſe

these two places both in longitude and latitude. This situation is very different from what the moderns supposed; the difference of the meridians between these two cities is found to be less by 45 than they thought, and on the contrary their difference in latitude is found to be more by 20′; and yet the distance between these two cities in both *hypotheses* is the same; but as the moderns suppose that the degree contained 60 ancient miles instead of 75, they cannot bring to an agreement therewith the distances of the road named *Via-Cassia*, which led from *Rome* to *Florence*; they are therefore obliged to alter the distances, and when *Strabo*, *lib.* 5. says the city of *Arezzo* is at 1000 *stadia*, or 120 miles from *Rome* toward mount *Appennine*, *Cluverius* would have us believe, that this distance must be taken from mount *Appenine*, and not from the very city of *Arezzo*; whereas by our measure of the ancient miles reduced to their value, we explain this passage of *Strabo*, and the ancient itineraries strictly, which shews their exactness and conformity with the observations of the academy.

The moderns have not explained the ancients better in the description which they have made of the famous *Appian* way, which was said to have been continued by *Caius Cæsar* from *Capua* to *Brundusium*. *Strabo* says this last city was distant from *Rome* by this road 360 miles, thus tho' *Cluverius* places between these two cities almost the same difference in longitude as results from the observations, yet this author, not being able to make his *hypothesis* of the measures agree with this distance related by *Strabo*, nor with those of the ancient itineraries, has altered them $\frac{1}{5}$, that is, so far as his measure differed from the true one.

It is upon the same principles, that *Cluverius*, who besides his learning, had the advantage of

ſeeing with his own eyes the countries in queſtion, having found in the town of *Polla* in the kingdom of *Naples*, an inſcription which related the diſtances of this branch of the *Appian* way, which leads to *Regio*, and finding theſe diſtances oppoſite to the notion he had formed of the meaſures of the ancients, endeavoured to render this inſcription ſuſpected, tho' it appeared very authentic to *Holſtenius*, who ſaw it as well as he. The words are *viam feci ab Regio ad Capuam, & in ea via pontes omnes, milliarios, tabellarioſque poſivi, hincce ſunt Nuceriam millia quinquaginta unum, Capuam octoginta quatuor.* And ſo of the other places of this road, the diſtances of which are marked, and theſe diſtances, of which the greateſt part are confirmed by the itinerary of the emperor *Antoninus*, concur with the ſituation of this country, by reducing, as I have done, the ancient miles to their true value.

The moderns have not only been miſtaken, when they have endeavoured to contradict the ancients, but they have alſo explained them amiſs, when they have attempted to follow them. *Lombardy*, which anſwers for the moſt part to the country called by the ancients *Gallia togata*, is croſſed in a right line by the famous *Emilian* way and by other roads, of which the diſtances are marked, not only in the *Theodoſian* table, which is the only geographical map remaining of antiquity, but alſo in the itinerary of *Antoninus*, and of that from *Bourdeaux* to *Jeruſalem*, of which the author is not known. The moderns have endeavoured to follow in theſe places the diſtances of the ancients, but they have fallen into another inconvenience; for regulating by theſe diſtances the longitudes of the cities by the falſe notion they had of the ancient meaſures, they have given too great

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an extent to this country with regard to the circumference of the earth, putting 5° 25′ in longitude from *Nice* to *Bologna*, whereas by the observations made at these two cities by the academy, there are but 4° 13′, which is $\frac{1}{5}$ less; and as we have shewn that the ancient mile was $\frac{1}{5}$ less than the moderns suppose it, it is plain that this important correction of the length of *Lombardy*, founded upon the observations of the academy, returns perfectly to the measures of the ancients, contrary to the opinion of the moderns.

It follows from thence, that the extent of the same country from N. to S. which the moderns have also regulated after their manner by the ancient measures, must be $\frac{1}{5}$ smaller by the observations, if it is true as I have advanced, that these observations are conformable to the measures of the ancients. M. *Cassini* has observed the latitude of *Genoa* to be 44° 25′, and M. *Petit* that of 46° 10′ at *Trabona* in the *Vallis Tellina*, and the result of these two observations gives also $\frac{1}{5}$ less between the parallels of these two cities, which is exactly what the moderns have assigned too much. We may see by the map, that the situation of *Genoa* given by the observations is very far from that which the moderns assign it; but how extraordinary soever this correction may appear, it appears still more surprising, that the measures of the ancients should square with it, and that our modern authors have departed so far from it.

The third fault of the moderns is, their not having had, as it seems, any regard in certain points to the measures of the ancients; and this fault is such, that whereas the former made them extend certain countries too much, this on the contrary has made them contract others too much. We may see by the map, that the distance from

Rome

Rome to the *Adriatic-ſea* is much greater according to the obſervations, than according to the moderns. The academy has not had an opportunity of obſerving in the neighbourhood of this ſea, but F. *Viva* has obſerved the height of the pole at *Loretto* to be 43 degrees, 42 minutes, and the geometrical operations of F. *Riccioli*, rectified by the obſervations of the academy, give us the longitude and latitude of *Ravenna*, as it is marked in my map.

The *Romans* had cauſed 3 fine roads to be made, leading from *Rome* to this ſea. The *Flaminian* way the length of which was 185 miles, according to *Antoninus*, to the city of *Fano*, formerly called *Fanum Fortunæ.* The ſecond road was the *Salarian* way, ſo called from the ſalt which was brought along it. This road, after having paſſed a place then called *ad centeſimum*, becauſe it was at the 100th ſtone, or 100 miles from *Rome*, ended at 32 miles from thence, at the city of *Truentum* on the ſame ſea. Laſtly, the third road was the *Valerian* way, upon which *Antoninus* reckons 140 miles to the mouth of the river *Aternus*. The diſtances, which agree ſo well with the reſult of the obſervations above related, differ throughout about 45 miles from thoſe of our moderns, and conſequently prove them to be miſtaken.

The obſervations favour alſo a paſſage in *Pliny*, which could not be explained in the *hypotheſis* of the modern geographers. This author ſpeaking of the different breadths of *Italy*, from one ſea to the other, ſays, that the city of *Oſtia*, at the mouth of the *Tyber*, is farther from the *Adriatic-ſea*, than the port of *Alſium*, on the *Tuſcan* coaſt. The moderns falſly ſuppoſe, that the coaſt between the mouths of the *Tyber* and of *Alſium*, runs to the weſt; thus receding more and more from the

Adri-

Adriatic ſea. But by making this coaſt run toward the north, as I have done, the paſſage of *Pliny* is literally explained. And it is not at random that I have thus marked it, for it is found the ſame in the exact ſurvey, that has been made at *Rome* of the *Annonian* lands; and this correction is alſo confirmed in the groſs by an obſervation, which M. *Chazelles* has made at *Civita Vecchia*, which is ſituated on the ſame coaſt; for he has found by this obſervation, that this city was 13′ more to the north than *Rome*, whereas the moderns make it 8′ more to the ſouth.

I could prove by a great many other examples, the conformity between the obſervations of the academy, and the meaſures of the ancients, and juſtify them with regard to ſeveral miſtakes that have been falſely aſcribed to them; but as this would draw the preſent diſcourſe into too great a length, I ſhall content myſelf with adding ſome examples, which ſhew, that where obſervations are wanting, we may venture to uſe the meaſures of the antients.

The ſituation of *Carthage* ſhall be the firſt of theſe examples; and it is ſo much the more conſiderable, as inſtead of 20 leagues in 100, to which the former errors at moſt amounted, this is 100 leagues in 220.

The modern authors make 90 leagues from *Lilybæum*, a promontory of *Sicily*, to the city of *Carthage*, and 60 from the cape of *Mercury* in *Africa*, to the ſame promontory. But I find in the itinerary of *Antoninus*, that from the iſland called *Maritima* near *Lilybæum*, to the cape of *Mercury*, the diſtance is but 700 *ſtadia*. Now this diſtance is ſmaller by $\frac{2}{3}$ than what the moderns ſuppoſe between *Sicily* and *Africa*; and it is

is confirmed in general by the obſervations of the academy. For tho' there is no immediate obſervation in theſe 2 terms, that of F. *Feuillée*, at *Tripoly*, in *Barbary*, and that of M. *de Chazelles*, at *Trapano*, near *Lilybæum*, ſhew, that the coaſts of *Africa*, and conſequently the city of *Carthage*, which was there ſituated, are 3 degrees more to the north, and that they are much nearer to *Sicily*, than the moderns ſuppoſe.

Beſides the ſailors agree, that from cape *Bona*, which is the modern name of the cape of *Mercury*, to *Lilybæum*, there are but 19 or 20 ſea-leagues, which amount to the 700 *ſtadia* of *Antoninus*.

This exceſſive error of the moderns, contrary to the opinion of the antients, in the ſituation of ſo famous a city as *Carthage*, is again very viſible, by the diſtance between the iſland *Sardinia*, and *Africa*, related in the ſame itinerary of *Antoninus*. He makes from *Cagliari*, the capital of *Sardinia*, to *Carthage*, only 1500 *ſtadia*. Now this diſtance valued in miles and leagues, is leſs by half than what the moderns ſuppoſe; and this proof is ſo much the more concluſive for the ſituation of *Carthage*, as we have two obſervations from F. *Feuillée* at the ſouth coaſt of *Sardinia*, where *Cagliari* is ſituated, and as theſe obſervations determine the latitude of it.

Strabo, *Solinus*, and *Valerius Maximus*, relate, that there was a man, during the *Punic* wars, whoſe ſight was ſo good and ſo long, that from an eminence near the cape of *Lilybæum*, he counted all the veſſels, that ſailed from the port of *Carthage*, and gave notice thereof to the inhabitants of the city of *Lilybæum*.

Should we ſuppoſe, as the moderns inſinuate, that this man ſaw the veſſels only at the cape of *Mer-*

Mercury, which is near to *Sicily*, they could not explain this paſſage; for ſeeing they ſuppoſe, as I have already ſaid, 60 leagues between cape *Lilybæum* and the cape of *Mercury*, it is not only probable that a man could diſtinguiſh veſſels at ſo great a diſtance; but even had his eye ſight been good enough, the roundneſs of the earth would have concealed theſe objects from him.

But the fact related by theſe authors of this long ſighted man of *Lilybæum*, no longer appears ſo incredible by the reduction of this diſtance of 60 leagues to that of 20, and we might admit this ſingularity with more eaſe; but I do not inſiſt upon it.

Here is another eſſential difference, wherein the moderns are equally miſtaken by departing from the ancients. *Pliny*, *lib.* iii, *cap.* ii, ſays, that the city of *Otranto* is ſeparated from the coaſt of *Greece* only by a ſtreight of 50 miles in breadth, and that *Pyrrhus*, king of *Epirus*, had thoughts of building a bridge over it, when he was called thither by the *Tarentines*, to make war againſt the *Romans*, but that he was diverted from doing it by other cares.

I do not pretend to juſtify the poſſibility of the deſign of this ambitious and viſionary prince, I ſhall only endeavour to eſtabliſh the breadth of this ſtreight according to the antients, and to ſhew, contrary to the opinion of the moderns, that this diſtance is the true one.

The itinerary of *Antoninus* makes 400 *ſtadia* between *Otranto* and the iſland of *Saſina*, on the *Grecian* coaſt. Theſe 400 *ſtadia* valued in miles, make juſt the 50 miles which *Pliny* reckons between the 2 lands.

We have no obſervations in theſe places, but for want of them I have had recourſe to the charts, the diſtances of which are ſo much the more exact, as they have been ſet down on the relation of an infinite number of pilots. Theſe charts agree in the diſtance of 12 ſea-leagues between the city of *Otranto* and *Saſna*; and this diſtance perfectly agrees with that which *Pliny* and *Antoninus* mark between theſe 2 lands; and yet our moderns make this ſpace as great again. Thus we may truly ſay, that the meaſures of the antients may juſtly be uſed to bring our knowledge to perfection; eſpecially as theſe meaſures agree with what we have, that is beſt and moſt certain. I ſhall conclude with a compariſon of the eaſtern and ſouthern part of *Italy*, called by the ancients *Magna Græcia*, with *Greece* properly ſo called.

The *Greeks* had ſent ſo great a number of colonies into this part of *Italy*, that it was called *Greece*, as well as the country, which had born that name in all ages. But the moderns comparing the extent of this country with that of *Greece* properly ſo called, which contained *Achaia*, *Peloponeſus*, and *Theſſaly*, fancied the name of *Magna Græcia* to agree better with this ancient *Greece*, which was larger than the other according to their *hypotheſes*. Theſe moderns therefore, and *Cellarius* among the reſt, not knowing how to explain the ancients in this particular, aſcribe this pretended error of the ancients to the vanity of the *Greeks*, but I ſhall ſhew they are juſtified by the obſervations.

F. *Feuillée*, in concert with the gentlemen of the obſervatory, has obſerved the heights of the pole, and the longitudes of *Theſſalonia*, *Milo*, and *Candia*. I have alſo collected the obſervations of Mr.

Mr. *Vernon*, an *English* gentleman at *Lacedæmon*, *Athens*, *Thebes*, *Corinth*, *Chalcis*, and other places It results from all these observations, that the length which before had been given to *Greece*, properly so called, as well as its breadth, exceeded the truth by several degrees, so that this country is found to be less by $\frac{1}{2}$ than was supposed, as may be seen by the map, where this country appears by the observations, less than that which was called *Magna Græcia*, conformably to the idea which the ancients have given us of it.

I could also justify, by the measures of the ancients, this extent of the ancient *Greece*, so different from that which has at present been given it; and to shew the conformity of the measures of the *Greeks* with the observations, as I have also proved the agreement of these observations with the measures of the *Romans*, but the discussion of this deserves a particular dissertation. It is sufficient for me here to have justified the idea, which the ancient *Greeks* have given us of this part of *Italy*, to which they sent colonies, and that it was not out of vanity that they called it *Magna*, or *Great*, since this name is founded upon truth.

IV. *Observations to determine the difference between* Paris *and* Leyden, *and between* Paris *and* Upsal; *by* M. Maraldi*.

M. *Sombac* has made at *Leyden* some observations on the eclipses of the satellites of *Jupiter*, which being compared with those made at the same time at *Paris*, shew the difference of the meridians between these 2 cities, and the meridian height of the lion's heart, which he has observed,

* July 11, 1714.

ſerves to determine the height of the pole at *Leyden.*

To know the true hour of the eclipſes of the ſatellites of *Jupiter*, M. *Sombac* has made ſeveral days ſucceſſively with a pendulum of ſeconds, the obſervations of the paſſage of the lion's heart over the meridian, which being compared together, ſhew, that the pendulum was regulated by the mean motion. The ſame obſervations added to the right aſcenſion of the lion's heart, and to that of the ſun for each day, give the ſtate of the clock with regard to the true hour.

Theſe are the particulars which we have received *April* 3, 1707, the lion's heart paſſed over the meridian at $9^h\ 6'\ 52''$ by the clock, its meridian height being $51^\circ\ 14'\ 40''$. M. *Sombac* obſerves, that after this obſervation there happened ſome accident to the needle of the ſeconds.

April 4, the ſun arrived at the meridian, the clock pointing $12^h\ 3'\ 16''$, and the evening of the ſame day the lion's heart paſſed over the meridian at $9^h\ 2'\ 58''$. To find the true hour of this paſſage, we have calculated the right aſcenſion of the ſun for 9^h at *Leyden* $13^\circ\ 16'\ 30''$, which being compared with the right aſcenſion of the lion's heart $148^\circ\ 11'\ 35''$, will give the difference of the right aſcenſion $134^\circ\ 55'\ 5''$; this difference converted into hours at the rate of 15° for an hour, makes $8^h\ 59'\ 40''$, the true hour of the paſſage of the lion's heart over the meridian. This hour being compared with that obſerved, which is $9^h\ 2'\ 58''$, we ſhall have for difference $3'\ 18''$, which the pendulum anticipated with regard to the true hour; which agrees with the obſervation of the ſun made at the meridian the ſame day by M. *Sombac.*

April 5, M. *Sombac* obſerved the emerſion of the firſt ſatellite of *Jupiter* at $2^h\ 37'\ 16''$ in the morn-

morning, at which time the pendulum accelerated $3'\ 5''$ with regard to the true hour; which being substracted from $2^h\ 37'\ 16''$ gives the true hour of the emersion at $2^h\ 34\ 11$. As this eclipse could not be observed at *Paris*, we shall compare two others, which were observed at both places.

April 13, at *Leyden*, the heart of the lion passed over the meridian at $8^h\ 27'\ 22''$. By the right ascension of the sun compared with that of the lion's heart, we have calculated the hour of this passage at $8^h\ 27'$, therefore the clock anticipated $22''$ with regard to the true hour. The same day M. *Sombac* observed exactly the emersion of the first satellite of *Jupiter* at $10^h\ 57'\ 20''$. The next day *April* 14 he observed the lion's heart at the meridian, at 8 23 27; but by the astronomical calculation the true hour of this passage ought to be at $8\ 23'\ 21''$. Therefore the clock anticipated $6''$ with regard to the true time. We have found that the preceding day, *April* 13, the anticipation of the clock with regard to the true time was $22''$, therefore the clock loses $16''$ in a day, with regard to the true time, which is at the rate of $1''$ in an hour and a half; from the hour of the passage of the lion's heart observed *April* 13, to the hour of the emersion, there are $2\ 30'$, in which time the clock has lost $2''$ with regard to the true hour; but we have found the anticipation of the clock $22''$ at the hour of the passage of the lion's heart, therefore at the hour of the emersion the anticipation of the clock with regard to the true hour was $20''$, which being substracted from the hour of the emersion observed the 13th, at $10^h\ 57'\ 20''$ gives the true hour of the emersion at *Leyden* for $10\ 57'$. It was observed the same day at *Paris* at $10^h\ 48'\ 4''$, the difference of the meridians is $8'\ 56''$ of time, which make $2^\circ\ 14'$, by which *Leyden* is more eastward than *Paris*.

April

April 29 the passage of the lion's heart over the meridian was observed at $7^h\ 24'\ 16''$; and by the calculation it should be at $7^h\ 27'\ 30''$, therefore the clock was $3'\ 14$ too slow with regard to the true hour, which being added to the hour of the emersion observed at *Leyden*, the same day at $9^h\ 16'15''$ gives the true hour of the emersion at $9^h\ 19'\ 29''$; it was observed at *Paris* at $9^h\ 10'\ 20''$, the difference of the meridians is $9\ 7''$ which make $2^\circ\ 17'\frac{1}{4}$ by which *Leyden* is more eastward than *Paris*. By the preceding observation it amounts to $2^\circ\ 14'$, which we had better keep to, because M. *Sombac* marks this more exactly than the last.

	°	′	″
The meridian height of the lion's heart was observed at *Leyden* in 1706, to be	51	14	40
The refraction to be substracted 48, therefore the true height	51	13	52
Northern declination of the lion's heart to be substracted for 1706	13	22	0
Remains the height of the equinoctial at *Leyden*	37	51	52
And the complement to 90° is the height of the pole	52	8	8
M. *Sombac* makes it	52	10	

We have an observation of the eclipse of the moon made at *Upsal*, *October* 21, 1706. This city is in *Sweden*, 12 leagues from *Stockholm* towards the N. W. the observation was made with a clock with seconds regulated at the noon of the day preceding the eclipse, during which the sky was continually clear. For the spots, the observer, whose name we could not know, made use of the denomination which has been marked by *Hevelius*, and he used a telescope of 3 feet. Thit is the observation just as we received it.

h	′	
7	12	*Initium umbræ.*
7	20	*Sinus porphyrites immergitur.*
7	40	*Ætna mons.*
7	48	*Palus Mœotis.*
8	8	*Mare Ægyptium.*
9	4	*Ætna emergit.*
9	23	*Propontis insula.*
9	38	*Totus pontus Euxinus.*
9	48	*Palus Mœotis emergit.*
9	50	*Finus umbræ.*

Comparing the beginning with the end of this eclipse, we have its duration $2^h\ 38'$, and the middle at $8^h\ 31'$. We could not make the observation of this eclipse at *Paris*, because the heavens were not favourable; and we have no observation of its beginning from any place, the sky not being clear 'till towards the end, where a part of this eclipse was observed. We shall therefore compare the *Upsal* observations with those which were made at *Marseilles*, by M. *Chazelles* and F. *Laval*, which are related in the memoirs of 1706.

At $9^h\ 4'$ at *Upsal* *Ætna* emerges; this spot is that which *Riccioli* calls *Copernicus*; it entirely emerged at *Marseilles* at $8^h\ 6\ 21''$ therefore the difference of the meridians between *Upsal* and *Marseilles* is $57'\ 39''$.

At $9^h\ 48'$ at *Upsal* the whole *Palus Mœotis* emmerges; this spot is what is called the *Caspian*. By the observation at *Marseilles* this whole spot emmerged at $8^h\ 50'\ 16''$; therefore the difference of the meridians between *Upsal* and *Marseilles* is $57'\ 44''$; by which *Upsal* is more eastward than *Marseilles*.

The end of the eclipse was observed at *Upsal* at $9^h\ 50'$; at *Marseilles* it was observed at $8^h\ 52'\ 16''$, the difference of the meridians is $57'\ 44''$, by

by which *Upſal* ſhould be more eaſtward than *Marſeilles.* Theſe 3 different compariſons agree in giving the ſame difference of the meridians within about 5″.

By a great number of obſervations, we have found the difference of the meridians between the obſervatory and *Marſeilles* 12′ 30″, which added to 57′ 44″, the difference between *Marſeilles* and *Upſal*, we ſhall have 1^{h} 10′ 14″ for the difference of the meridians between *Paris* and *Upſal* which make 17° 33′ 30″ by which *Upſal* is more eaſtward than *P ris.*

This obſervation added to others, which have been made near the *Baltick* ſea, will ſerve to determine a part of its different ſinuoſities.

V. *Obſervations on a very ſingular ſpecies of aquatic-worm, by M.* de Reaumur*; *tranſlated by Mr.* Chambers.

It is no wonder the worm I am about to ſpeak of ſhould hitherto have eſcaped the obſervation of naturaliſts, as being rare, very ſmall, and at firſt ſight preſenting nothing very extraordinary, yet upon a nearer inſpection we find it highly deſerving of ſome attention, the ſmalleſt inſect comes from the ſame hand as the largeſt animals, and all equally bear the character of the great maker.

Our water-worm is not above 7 or 8 lines in length, and yet ſeems to make a claſs of itſelf, at leaſt we know of no claſs of animals under which it may be ranged. Terreſtrial animals live on the land, aquatics in water, and the amphibious kind ſometimes on land, and ſometimes in water. In this the 2 extremities of the body are aquatic, its head and tail being always in water; yet the reſt of its

* June 20 1714,

body always on land to conceive how this may be its figure, muſt be deſcribed.

Like ſeveral other inſects it conſiſts of *annuli*, eleven whereof are told between the head and the tail, all of them nearly ſpherical, and repreſenting beads of a chaplet, ſtrung one cloſe to another. The inſect is almoſt always bent or folded in two, like a ſiphon, one of its parts being longer than the other, yet almoſt parallel thereto, ſo that the head and tail are always near together. The part between the bend and the head is ſhorter than that between the ſame bend and the tail, yet the angle of flexure uſually falls upon the 6th *annulus*, but the 5 *annuli* towards the tail are larger than the other 5 towards the head.

'Tis only its head, tail, and the *annulus* next the tail, that are conſtantly in water; the 9 other *annuli*, or at leaſt 7 of them, are upon land; accordingly the inſect always reſides at the brink of ſtill waters, a turbulent water by no means ſuiting it. As ſoon as the water covers it a little further than as above-mentioned, it grows uneaſy, and gets further off: on the contrary, if the water leave leſs of it covered, it immediately draws nearer.

The worm not being above 8 lines long, this obſervation would be difficult to make when it is on the brink of a large water. Accordingly it was by examining it in cups or glaſſes of water, that I made the diſcovery. I found it always faſtened to the *parietes* of the veſſel in ſuch manner, that its head and tail were in the water, and the reſt of its body out of it; and when by inclining the veſſel on one ſide, I obliged the water to cover more, the animal inſtantly drew back, with all the expedition poſſible; and when inclin-

ing the veſſel a contrary way, the water relinquiſhed it to run eagerly in purſuit thereof.

Indeed it was its way of walking, or creeping, that firſt gave me occaſion to examine it more cloſely, as ſeeming to deſerve a place among the progreſſive motions of water animals already treated of in ſeveral memoirs. In its natural way of walking, it is the middle of its body that goes firſt towards the place it is bound for, that is, the ſixth *annulus* is the foremoſt, and like the head in quadrupeds, leads up the reſt of the body. In a word, the worm when walking, remains bent, like a ſiphon; and it is the *annulus* in the middle of the bend that goes foremoſt; nor does it advance thus by a vermicular motion, but as legs, though ſmall ones indeed, and ſuch as are ſcarce viſible without a magnifier.

Its legs are in effect one of its ſingularities; they grow from its back, that is, from the ſide oppoſite to its belly, taking its belly after the ſame manner as in caterpillars, *millepedes*, and other inſects, which bear a reſemblance to this in figure, *viz.* for the ſide towards which are its apertures of the *anus* and mouth; and to which the head is uſually inclined. Hence it follows, that this inſect conſtantly lies on its back, as others do on their belly, and that its mouth looks upwards; this laſt circumſtance is not peculiar to it, there being ſeveral other ſpecies of water inſects and flies, that ſwim on their back, as being to live upon inſects which ſwim or walk on the ſurface of the water. And it will hereafter appear, that there was a like reaſon for its mouth being always upwards. But to return to its legs:

There are ten of theſe placed by two and two upon each *annulus*, between the 6th *annulus* and the head we only find four, but theſe are bigger

than

than the ſix other; the two firſt are towards the extremity of the third *annulus*, and the two others towards that of the fourth or beginning of the fifth, the third pair is towards the beginning of the eighth *annulus*, the fourth pair on the ninth, and the fifth on the tenth; theſe legs are all very ſhort, nearly reſembling the hind legs of caterpillars, or thoſe of ſilk-worms, and having their extremities like them furniſhed with a kind of hooks or claws.

The four firſt legs, that is, thoſe between the head and ſixth *annulus*, are inclined towards the head, and the reſt towards the tail: hence when the animal is folded in two, its legs though differently inclined with regard to its head, are all turned the ſame way, and diſpoſed in the ſame manner with regard to the ſixth *annulus*; and conſequently may all concur to make this *annulus* advance towards a certain point; as the legs of other animals make their head advance.

'Tis eaſy to conceive, how theſe legs whoſe extremities are turned towards the head and tail of the worm, ſerve to make it move; it need only ſtretch its legs backwards or towards the middle *annulus*, and then thruſt itſelf on by their means, and it will walk forwards; if it would go backwards or make its head and tail go foremoſt, its legs are of no uſe, nor has it any thing to truſt to in this caſe, but its vermicular motion, for which reaſon it rarely moves in this manner.

Beſides the motions above mentioned, it can execute two others by means of its legs; it can move ſideways, by reaſon the legs are not only moveable forwards and backwards, but likewiſe from right to left, and from left to right, theſe two motions it ſometimes makes uſe of, when it would go to places very near at hand.

When immerged intirely in water it ſtretches itſelf out at full length, and ſwims like other worms bending and waving its body various ways.

But it never ſeems to commit itſelf to full water unleſs when forced thereto, and in that caſe quickly gets to the ſide and lodges itſelf there, with its mouth upwards; the mouth is ſurrounded with four little hooks like thoſe of predacious inſects, and from its middle ariſe two other little parts in the manner of tufts; the animal is continually agitating theſe little tufts, ſometimes lengthning, then contracting them, now moving them to the right, and then to the left, by which agitation the water is kept in motion; it alſo ſeems as if it attracted the water ſomewhat after the manner of reſpiration; at leaſt this is certain, that while its tufts are thus agitated, the little bodies floating in the water come from afar off and run into its mouth, which is the ſtratagem it makes uſe of to feed itſelf; when it has attracted ſome little body which it judges a proper food it ſtretches out its head ſeizes it greedily and ſwallows it down. I have ſeen it catch inſects ſo ſurprizingly ſmall, as ſcarce to be viſible with an excellent magnifier, after this manner tho' all it can take this way be very little, yet it eats a great deal in proportion to its bulk, one little body or other being continually entring its mouth, the greateſt part of them we may ſuppoſe are not proper food, accordingly we frequently find it caſting out excrements, at leaſt there is a viſible diſcharge of pretty conſiſtent greeniſh filaments, through an aperture near its tail. Its belly is browner than its back, and the *annuli* in this part have a circular line ſurrounded with hairs, like thoſe at the apertures of the *tracheæ* in ſome terreſtial inſects, and 'tis not improbable but this may likewiſe breath an air.

The

The *annuli* have no ſuch line on the back, but are white, tranſparent and ſoft, and let us ſee ſeveral motions carried on in its body; near its *anus*, is a circular tube which alternatively removes from and approaches to the *anus*, like the piſton of a ſyringe, when after ſwelling more than ordinary the worm flattens again, its excrements may be ſeen to iſſue forth; I ſhould have been inclined to ſuppoſe that by this motion it drew in the water, but could never ſee any enter but by the mouth; there is a ſimilar motion about the third *annulus*, in a tube which ſeems to be a continuation of the former, and 'tis probable the heart of the animal is placed hereabout——————But this is enough for ſuch a diminutive inſect, which furniſhes us with the firſt inſtance of a creature, which naturally lives with its head and tail in water, and the reſt of its body on land, which has its legs on its back, and which walks with the middle of its body foremoſt, as other animals do with their head foremoſt.

VI. *Reflections on ſome new obſervations on the tides made in the port of* Breſt; *by M.* Caſſini*.

The obſervations on the tides, which have hitherto been made in various ports of *France*, have ſhewn us what relation there is between the motions of the moon and thoſe of the ſea; all the inequalities obſerved in this planet upon the ſea ſeem to be repreſented, and notwithſtanding the agitation in which it is continually found by the impulſe of winds againſt the coaſts, the direction of which reflects its waters different ways, yet we always find in its motion a period following the different ſituations of the moon with regard to the earth.

* Auguſt 4, 1714.

It

It ſtill remained to explain ſome of theſe inequalities, which at firſt ſeemed ſuſceptible of no rule, but we have ſince found, that we might aſcribe them to the action of the ſun, ſo that the different combinations of the motion of theſe two planets, and of their ſituation towards each other and with regard to the earth, produce moſt of the varieties obſerved in the tides.

As for the ſun it finiſhes its courſe in a year, after which the inequalities found in its motion return nearly in the ſame order. Its *apogeum* moves very ſlowly, ſo that the direction of its orb does not vary ſenſibly in ſeveral years. It is not the ſame with the motions of the moon; its revolution is much quicker than that of the ſun, and it is ſubject to a much greater number of inequalities. Its apogeum, and conſequently the direction of its orb, vary conſiderably in the ſpace of a year; its nodes or the interſection of its orb with the ecliptic have alſo a motion the contrary way, which produces in its ſituation with regard to the earth ſome varieties, which cannot be obſerved but in a long ſeries of years. It was important therefore to have a great number of obſervations of the tides ſuch as we have hitherto had, for which we are indebted to the orders of M. *le Comte de Pontchartrain*.

In the interval contained between theſe laſt obſervations, there are 25 new and full moons, and a like number of quadratures, and they have obſerved the tides of two equinoxes and two ſolſtices. But before we examine them it muſt be obſerved, that ſun-dial hitherto uſed at *Breſt* to regulate the pendulum, did not mark the noon exactly, and that M. *Coubard*, profeſſor of hydrography in this port, having drawn a meridian line with a great deal of care, had found that it advanced

ced upon thet rue hour 17′, which muſt be ſubſtracted from all the obſervations that have been ſent us.

This difference, which is pretty conſiderable, changes the mean hour of high-water, which we had determined at *Breſt*, inthe new and full moons, at 3^h 45′, and in the quadratures at 8^h 57′; but it cauſes no variation in the method that we had given, to determine the height of the full ſea, nor in the progreſſion with which the tides increaſe or diminiſh. For if we ſubſtract 17′ from theſe times, thus determined, we ſhall have the mean time of the full ſea at *Breſt*, in the new and full moons at 3^h 28′, or $3^h \frac{1}{2}$, and in the quadratures at 8^h 40′; and ſuppoſing the rule, which has been preſcribed in the laſt memoirs, we ſhall find the true time of the full ſea in all the phaſes of the moon with as much exactneſs as has been done hitherto.

For example; *June* 8, 1713, the day of the full moon, which happened at 6^h 21′ P. M. the full ſea in the morning was obſerved at 2^h 53′, corrected time, which is one of thoſe that happened the ſooneſt in the ſpace of three years. Suppoſing the mean time of the full ſea at *Breſt* at $3^h \frac{1}{2}$, we ſhall find that this full ſea muſt have happened at 3^h in the morning, within about 7′ of the obſervation.

In like manner, *Dec.* 2, 1713, the day of the full moon, which happened at 3^h 30′ in the morning, the full ſea of the evening was obſerved at 4^h, corrected time. It ſhould have happened according to the rule at 3^h 54′, within about 6′ of the obſervation.

In theſe two examples the calculation of the tides agree with the obſervation within about 6 or 7′, which is but an inconſiderable difference, and may

may eaſily be aſcribed to the difficulty of determining exactly the hour of the full ſea. It is true, there are ſome obſervations, which differ more from the calculation; but theſe apparent inequalities are nevertheleſs ſuſceptible of ſome rules, for we obſerve, that when the tides are very great, the full ſea happens early, and anticipates the calculation; and that quite contrary it happens later, and retards with regard to the calculation, when the tides are very ſmall.

For example, *Jan.* 11, 1713, the day of the full moon, the height of the full ſea was obſerved 15 feet, 10 inches, 6 lines, one of the ſmalleſt that has been obſerved; alſo the time of the ſea happened at 3^h 46′ in the evening, later by 26′ than according to the calculation.

On the contrary, *Feb.* 14, 1714, the day of the new moon, the height of the full ſea was obſerved in the morning, 18 feet 5 inches, one of the greateſt that has been obſerved; alſo the time of the full ſea happened at 2^h 58′, 10 minutes ſooner than according to the calculation. It is the ſame with moſt of the other obſervations, ſo that we may uſe an equation to correct the times of the tides, according to which they ought to be greater or leſs, and find the hour of the full ſea with more exactneſs than had hitherto been done.

We ſhall here obſerve, that in *Sept.* 1713, the full moon being marked in the *connoiſſance des temps*, and in the *ephemerides* of *Beaulieu*, the 5th at 5^h P. M. the height of the morning tide was calculated for that day at 3^h 4′, 37′ ſooner than according to the obſervation; and that of the evening at 3^h 28′, 40′ ſooner than what had been obſerved. This difference, which was greater than any of thoſe which we had obſerved, made us curious to cal-

calculate the true time of the full moon, and we found, that it ought to happen *Sept.* 4, at 5 in the evening, a day ſooner than had been marked; ſo that on this occaſion the tides have ſerved to diſcover the error which had ſlid into the calculation of one phaſe of the moon.

With regard to the time of the full ſea in the quadratures, we ſhall find it in the ſame manner as in the ſyzygies, tho' with a little leſs exactneſs. For *April* 17, 1713, the day of the laſt quarter, which is marked at 9^h 40′, P. M. the full ſea in the morning happened at 8^h 1′, which is one of thoſe that has been obſerved the ſooneſt. Suppoſing the mean time of the full ſea at *Breſt*, in the quadratures at 8^h 40′, we ſhall find that it muſt have have happened according to the calculation at 8^h 7′, 6′ later than what was obſerved.

May 17 following, the day of the laſt quarter, which is marked at 3^h 11′ in the morning, the full ſea happened at 9^h 51′, P. M. it ought to have happened according to the calculation at 9^h 24′, ſo that the time of this tide is found repreſented by means of this rule, with a difference only of 27′, inſtead of an hour, and 11 minutes, that there was between the mean time of the full ſea in the quadratures and the obſervation.

The mean time of the full ſea being at *Breſt*, in the *ſyzygiæ* at 3^h 30′, and in the quadratures at 8^h 40′, it follows, that the ſum of the retardations of the tide from the new and full moons to the quadratures is 5^h 10′, leſs by 1^h 40′, than the ſum of the retardations of the tides from the quadratures to the *ſyzygiæ*, which is 6^h 50′. This *phænomenon*, which we had firſt obſerved at *Dunkirk* and *Havre*, and afterwards at *Breſt*, appeared to us till now very difficult to explain. However, I think we may eaſily give the following reaſon for

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it, according to the received principles of all the philosophers.

We suppose, that the greater the effort is with which bodies are moved, the greater is their velocity. This is manifest in solid bodies, and we see the effects of it also in fluids, such as the waves of the sea, which the more they are agitated by the wind, move with greater impetuosity, by increasing in bulk at the same time. It follows from thence, that the greatest tides, which are caused by a greater pressure, must happen sooner, and that the retardations from one day to another, must be less than in the small tides, when the sea is agitated by a less strong pressure. That is really observed, conformably to what we have remarked, that in the *syzygiæ*, when the tides are greatest, the full sea happens earlier, and the retardation from one day to another is less than toward the quadratures, when the tides are smallest.

Thence it results, that the sum of the retardations of the tides from one *syzygia* to the next quadrature must be less than the sum of the retardations from this quadrature to the following *syzygia*. For if we consider, that the times of the greatest tides always happen in the interval contained between the *syzygia* and the quadrature, and that the time of the least tides always happens from the quadrature to the next *syzygia*, we shall find, that from the *syzygiæ* to the quadratures, the tides one with another are greater than from the quadratures to the *syzygiæ*; and that so the sum of the retardations of the tides which are, as has been said already, in a reciprocal *ratio* of the heights, must be less from the *syzygia* to the next quadrature, than from this quadrature to the following *syzygia*, according to experience. This will be made plain by an example.

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April 10, 1713, the day of the full moon, the height of the full ſea was obſerved in the evening 16 feet, 9 inches, 8 lines. It was the 11th, 17 feet, 1 inch; the 12th, 17 feet, 3 inches; the 13th, 17 feet, 2 inches; the 14th, 16 feet, 4 inches; the 15th, 14 feet; and the 16th, 14 feet, 3 inches. The ſum of all the heights of theſe tides to the quadrature, is 112 feet, 11 inches.

April 17, the day of the laſt quarter, the height of the full ſea was obſerved in the evening, 13 feet, 6 inches, 8 lines. It was the 18th, 13 feet, 1 inch; the 19th, 13 feet, 8 inches; the 20th, 13 feet, 5 inches; the 21ſt, 14 feet, 7 inches; the 22d, 15 feet, 4 inches; and the 23d, 16 feet, 7 inches. The ſum of the heights of all theſe tides to the new moon of *April* 24, is 100 feet, 3 inches, leſs by 12 feet, 8 inches, than from the full moon to the quadrature, and conſequently the ſum of the retardations of the tides, from the full moon of *April* 10, 1713, to *April* 17, the day of the laſt quarter, muſt have been leſs than from the laſt quarter to the new moon of *April* 24, according to experience, the retardation of the full ſea having been obſerved to be 5 hours, 18 minutes, from *April* 10 to 17, and 6 hours, 35 minutes, from the 17th to the 24th.

It appears therefore, that the difference between the retardations of the tides, from the *ſyzygiæ* to the quadratures, and the retardations of the tides from the quadratures to the *ſyzygiæ*, proceed from the terms of the greateſt and leaſt tides not happening in the *ſyzygiæ*, nor in the quadratures, but commonly 1 or 2 days after, which has been found in almoſt all the obſervations, and appears to be a proof, that the moon is the principle and cauſe of the tides. For this perfect correſpondence between the motions of the moon, and thoſe of

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the ſea, cannot be produced, but by ſome motion in the earth, which is the principle of the motions of the moon, or by ſome other unknown cauſe, which at the ſame time has an influence on the earth, and on the moon, or laſtly by the preſſure of the moon. If the tides were produced by ſome motion of the earth, the effect would make itſelf firſt be felt on the ſea, and afterwards communicate itſelf to the moon, thus the great and ſmall tides, and all the *phœnomena* depending thereon, far from following the phaſes of the moon, ought to precede them, which is contrary to experience. If it was one and the ſame cauſe, which acts at the ſame time upon the moon, and upon the ſea, the times of the tides would be in concert with the motions of the moon, and the different alterations therein obſerved, would be perceived at the ſame time with the different phaſes of this planet.

It remains therefore to conclude, that it is the moon which is the principle of the motion of the tides, ſince the preſſure which it cauſes, is not perceived in the inſtant, but ſome time afterwards, according to the ordinary laws of the motion which is communicated from one body to another, by the ſucceſſion of time.

To return to our obſervations, we obſerve in theſe as well as in the preceding, that the different diſtances of the moon from the earth contribute greatly to the different heights of the tides, and this cauſe is ſo conſtant, that of above 140 obſervations both in the ſyzygies and in the quadratures, that we have examined, there is not a ſingle one that has varied from it.

For example, *July* 22, 1713, the day of the new moon, this planet being then near its *apogeum*, and its diſtance from the earth 1064 parts, of which

which the mean is 1000, the height of the full ſea was obſerved 16 feet 1 inch, and *July* 25 in the evening, the day of the greateſt tide, it was found to be 16 feet 9 inches, which is one of the ſmalleſt that has been obſerved at *Breſt*. *Auguſt* 6, the day of the full moon, this planet being very near its *perigeum*, and its diſtance from the earth 935, the height of the full ſea was obſerved to be 19 feet 6 inches, and the next day in the evening the day of the greateſt tide, 20 feet 9 inches, greater by 3 feet 5 inches than in the preceding obſervation.

We find by the ſame reaſon, that when the diſtances of the moon from the earth are nearly equal between themſelves, the great tides of the new and full moons are alſo of the ſame height. For example, *April* 10, 1713, the day of the full moon, the diſtance of the moon from the earth being 1004, as well as *April* 24, the day of the new moon, they found *April* 12, the height of the greateſt tide to be 17 feet 3 inches; and *April* 25, 17 feet 4 inches, with a difference only of one inch between the obſervations of *April* 12 and 25.

This inequality of height in the tides cauſed by the different diſtance of the moon from the earth, is not the only one obſerved; for we have already remarked, that the nearer the moon was to the equinoctial, the greater were the tides, and that they diminiſhed in height in proportion as its declination increaſed. *March* 15, 1714, the day of the full moon, this planet being near the equinoctial, and its diſtance from the earth 939, they obſerved on the 17th in the morning, the height of the greateſt tide to be 19 feet 9 inches 6 lines above the fixt point, and that of the low-water 3 feet 6 inches below this point, ſo that the elevation of the tide that day was 23 feet 3 inches 6 lines, which

which is one of the greateſt that has been at *Breſt*; whereas *July* 8, 1713, the day of the full moon, its declination being 21 degrees S. and its diſtance from the earth 939, the ſame exactly as in the obſervation of *March* 15, 1714, the elevation of the tide was obſerved *July* 9, the day of the greateſt tide, 20 feet 5 inches 8 lines, leſs by near 3 feet, than *March* 17 of the following year.

In examining all the circumſtances of the obſervation of *March* 17, we find the ſun was then very near the equator, its ſouthern declination being but 2 degrees; ſo that three cauſes concurred to the height of this tide, namely the diſtance of the earth from the moon, which was then near its *perigeum*, and the ſituation of the ſun as well as that of the moon with regard to the equinoctial, from which theſe 2 planets declined a few degrees. It is true that the ſun was not then in its *perigeum*, its diſtance from the earth being 996 parts, of which the mean is 1000, but as the excentricity of the orb of the ſun is very ſmall, the effect of the diſtance of the ſun from the earth, which was greater only $\frac{1}{77}$ than when it is in its *perigeum*, could not be very ſenſible upon the tides, nor conſiderably diminiſh the greatneſs of them; for it muſt be obſerved, that the ſun in its *apogeum* is near the ſummer ſolſtice with a ſouthern declination; that towards the equinoxes, it is almoſt in its mean diſtance without declination, and that in the winter ſolſtice it is near its *perigeum* with a northern declination; the effect of the ſun upon the tides muſt therefore be the leaſt that is poſſible toward the ſummer ſolſtice. It muſt therefore be greater toward the winter ſolſtice, becauſe it is in its *perigeum*, but as the declination of the ſun is then very great, and as the action which reſults from its different declinations is ſtronger than that which

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is caused by the different distances of the sun from the earth, the effect upon the tides must be less than towards the equinoxes, when the sun is almost in the mean distances without declination. Thus the tide of *March* 17, this year should be very great, conformably to the observation.

It follows from what we have just related, that the tides of the *syzygiæ* must be the least, when the sun and moon are near their *apogeum*, and that these two planets are at the same time in their greatest declination with regard to the equinoctial, which is conformable to experience. For *June* 22, 1713, the day of the new moon, the distance of the moon from the earth being 1055, and its northern declination 23 degrees; the sun being also in its *apogeum*, with a northern declination of 23 ½ degrees, they observed *June* 23 and 24 in the evening, the height of the greatest tide 15 feet 10 inches, which is one of the lowest that has been observed at *Brest*. It is to be observed, that *June* 22, the day of the new moon, the height of the full sea was observed in the morning to be 15 feet, at night 15 feet 8 inches, lower than it was sometimes found on the day of the quadratures, when the tides are the smallest; which shews how important it is to know the rules of the different heights of the tides, for want of which the pilots might fall into the inconvenience of sometimes finding the height of the tides smaller in the *syzygiæ* than in the quadratures, and running against the coasts, and in different parts of the sea where there are banks of sand.

We have already observed, that the declination of the moon caused also some variety in the height of the two tides observed in the same day. That

in the ſummer *ſyzygiæ* the evening tide was greater than that of the morning, and that in the winter *ſyzygiæ* the morning tide was greater than that of the evening. That in the quadratures, which are about the vernal equinox, the morning tides are ſmaller than thoſe of the evening, and that on the contrary, about the autumnal equinox the evening tides are leſs than thoſe of the morning.

This is confirmed by theſe new obſervations; for *June* 8, 1713, the day of the new moon, the morning tide was 16 feet, 4 inches, leſs by 1 foot than the evening tide, and the greateſt tide happened *June* 10, in the evening. *June* 22, the day of the full moon, the morning tide was 15 feet, leſs by 8 inches than the evening, and the greateſt tide happened *June* 23, at night.

On the contrary, *Dec.* 17, the day of the new moon, the morning tide was 16 feet, 9 inches, 8 lines, greater by 1 inch, 8 lines, than that of the evening, and the greateſt tide happened the 20th in the morning.

Dec. 31, the day of the full moon, the morning tide was found to be 16 feet, 2 inches, greater by 4 inches, 6 lines, than that of the evening, and the greateſt tide happened *Jan.* 2, in the morning.

Alſo, on *Sept.* 12, and 28, and *Oct.* 12, 1713, the days of the laſt tides following the quadratures, the leaſt tides happened in the evening, and *Feb.* 23, *March* 10 and 24, 1714, the days of the laſt tides following the quadratures, the leaſt tides happened in the morning. It is obſervable, that in the winter *ſyzygiæ* the difference between the height of the morning and evening tide is leſs ſenſible than in thoſe of the ſummer; and that in the ſpring quadratures the difference between the morn-

morning and evening tide, is leſs than in thoſe of the autumn, which proceeds from the continual augmentation and diminution of the tides, the terms of which are 1 or 2 days after the *ſyzygiæ* and quadratures.

All theſe obſervations juſt related have ſuch regular periods, that we cannot doubt of their being produced by a certain principle. It appears alſo by the memoirs already given, that the rules preſcribed agree with the obſervations made at *Dunkirk* and *Havre de Grace*, except what is particular to each port, as the time of the full ſea in the phaſes of the moon, and the elevation of the tides, which is much more ſenſible in ſome places, than in others. Thus we muſt not confine theſe rules to the port of *Breſt*, and we may give a general inſtruction to pilots, which will ſerve to make them know the days of the greateſt and leaſt tides, provided care be taken to inſert in the *ephemerides* the apparent diameter of the moon for the day of each phaſe.

They will obſerve, 1ſt, The magnitude of the apparent diameter of the moon, for the greater it is, the more will the tides be elevated.

2dly, The declination of the moon, for the greater that is, the leſs are the tides elevated; and we find at *Breſt*, that the action of it is about $\frac{1}{2}$ of that which is cauſed by the different diſtances of the moon from the earth.

3dly, The declination of the ſun, which has the ſame effect with that of the moon, tho' with leſs force, which we judge to be nearly about $\frac{1}{2}$ that of the moon.

4thly, The magnitude of the apparent diameter of the ſun, the action of which is the leaſt ſenſible of all thoſe which we have obſerved.

When all these causes shall concur together; that is, when the diameters of the sun and moon shall be the least that is possible, and these 2 planets shall have their greater declinations with regard to the equinoctial, then the tides will be the smallest. But when the moon shall be *in perigeo*, and the sun and moon shall have no declination, in which case the sun will be about its mean distance, then the tide must be the greatest that is possible.

In the other situations of the moon with regard to the sun, the tides will be greater or smaller, according to a proportion which is usually deduced from the rules just prescribed, provided we know in each port the time of the full sea in the *syzygiæ*, and in the quadratures, and the greatest or least elevation of the tides.

VII. *Of the effects produced by the torpedo, or numb-fish, and the cause thereof, by M.* de Reaumur*; *translated by Mr.* Chambers.

There are few naturalists but have said something of the effect of the fish called in *Latin torpedo*, on such as touch it, the sciences have all of them certain shining topicks, which no body can well pass by, as in philosophy, the tides, and the properties of the magnet, and in natural history the *torpedo*.

Some, and especially among the ancients, speak of its effect with wonderous exaggerations, while others who have seen and handled the fish in some circumstances, without finding the numbness, represented all as a fable; but there has been no

* Nov. 14, 1714.

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room to doubt of it, ſince the atteſtation given the publick by *Redi* and *Borelli*, that they had experienced it. Though how well ſoever the fact ſeems aſcertained, the cauſe is not ſo.

Being on the coaſts of *Poitou*, in the fiſhing-ſeaſon for this animal, I reſolved to take it into examination, not with regard to the ſtructure of its body, there being a little treatiſe of S. *Lorenzini* printed at *Florence* in 1678, which has already exhauſted that matter: but my deſign was upon the cauſe of that numbneſs it produces in ſuch as touch it, wherein it preciſely conſiſts, and what are the circumſtances that attend it; for authors who agree as to the fact, diſagree about the circumſtances; ſome repreſenting the numbneſs as more violent, others as weaker; ſome inſiſting that the fiſh only produces it upon immediate contact, and others that its influence extends a-far off.———I ſhall only relate things as I experienced them; nor will I ſtick at a ſcrupulous detail of the circumſtances, which may perhaps be a means of diſcovery of the real cauſe of ſuch effect.

There are ſeveral ſpecies of this fiſh which we ſhall not ſtand to characteriſe, *Rondeletus* having done it to our hands.——To give a general notion of its figure to ſuch as have never ſeen it, we may compare it to a thorn-back, next to which it is commonly ranked by *Willoughby* and other writers, on the natural hiſtory of fiſhes. It has been already repreſented ſo often, that a draught of it might have been ſpared, which yet we have given in favour of ſuch as may not have books of fiſhes by them*, which ſhews the back-ſide of

* Plate I.

the fiſh, as taken very exactly from the life. Its ſize is various; thoſe uſually found on the coaſts of *France*, are about a foot and ½ long.

Having ordered ſome fiſhermen to ſave me what *torpedo's* they caught alive, they brought me a couple which were ſeemingly very lively and vigorous, and yet tho' I touched them in ſeveral places, and on the ſeveral circumſtances the leaſt numbneſs did not enſue. To rouze their courage I put them in veſſels full of ſea-water, where they ſwam about, and gave themſelves all the motions fiſhes uſually have in water; but ſtill I felt nothing extraordinary from them.

Yet the power this fiſh has of ſtriking numbneſs ſeemed ſupported by teſtimonies too authentick to be called in doubt: all therefore I concluded from this firſt experiment was, that my *torpedo's* were weakened, and had hereby loſt their virtue: to determine which I reſolved to examine them in the ſea itſelf, and accordingly engaged the fiſhermen to keep ſuch as they had caught therein, which was performed accordingly.

But the *torpedo's* ſeemed reſolved to make me doubt of their virtue; for the firſt I touched in the ſea, though a very large and vigorous one, and which had never been out of water, let me handle it ſeveral times without making me feel any thing unuſual. Upon this I only wanted a little more vivacity, to have concluded all that had been ſaid of it fabulous; when the *torpedo*, being at length fatigued with my repeated touches, let me ſee what it could do. A kind of numbneſs ſuddenly ſeizing my whole arm from hand to ſhoulder, and even affecting my head; it was very different from any common numbneſs, being accompanied with a great, though obtuſe pain.

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I was unable to ſtir my hand and arm, and felt a kind of ſhuddering through the whole, which it is impoſſible to repreſent. What comes neareſt it is, the painful ſenſation felt upon knocking the elbow violently againſt ſome hard body, which is the compariſon M. *Lorenzini*, *Borelli*, and others, have uſed, though it does not expreſs ½ the pain that accompanies this numbneſs, which I muſt confeſs was ſuch, that it a little ſlackened my zeal for making more experiments on the *torpedo*.

There were 5 or 6 other perſons with me, who had all the ſame deſire to touch the *torpedo* as myſelf, and were reſolved to make trial of its virtue, at the peril of the pain they were threatened with, and their curioſities were all rewarded like mine; the animal being now in the humour to make uſe of its power, few of them needed above one touch, eſpecially thoſe, who, like me, felt the numbneſs up to the ſhoulder; for there were 2 or 3 that came better off, the pain only reaching to the elbow.

The pain of this numbneſs, however, is of no long duration, but dwindles a-pace, and is intirely gone in a few moments. When my arm came to itſelf again, my curioſity returned for new experiments; accordingly I touched the *torpedo* again, and knowing the worſt, grew audacious, and touched it often. The numbneſs it now gave me I found did not go ſo far as the firſt, and withal proved much leſs painful; the reaſon probably was, that the *torpedo* had weakened itſelf by dealing ſo many blows, and the ſame might be the caſe with that of M. *Lorenzini*, which gave him no pain beyond the elbow; that of *Redi* on the contrary, which ſtruck a numbneſs up to his ſhoulder, muſt

muſt have been as vigorous as mine was at firſt.

But a learned *Engliſh* anatomiſt muſt have met with one much abler than any of them, if we may credit what he related before the grand duke of *Tuſcany*, unleſs we ſuppoſe, that the ſtrength of the animal may be heightened by any particular circumſtances. He affirmed, that the touch of the *torpedo* raiſed a pain in his arm which laſted 2 days. *Borelli* ſuſpects, that his imagination had increaſed the evil. But, may we not as well account for it from the ſtate of his body, for *Borelli* himſelf informs us, that the anatomiſt was ſeized with a paralytick *tremor*?

The matter of fact being eſtabliſhed, we proceed to enquire into the manner how it is effected. —— There have been 2 different accounts already advanced, for I make nothing of the third and antient one, which take up with aſcribing a torporifick virtue to the animal, not but this may be brought to much the ſame as one of the two other opinions, which aſcribes the effect of the *torpedo* to an infinite number of corpuſcules, which are continually iſſuing from its body, but more plentifully on ſome occaſions, than on others; this is the commonly received opinion, which has been adopted by Meſſ. *Redi*, *Perrault*, and *Lorenzini*, who explain themſelves thus; that as the fire emits a multitude of corpuſcules, fit to warm us, ſo the *torpedo* emits a like number of corpuſcules ſuited to numb the part they inſinuate into, either by their crowding too thick into the ſame, or by meeting with paſſages unſuitable to their figure.

The other account is *Borelli*'s, who looking upon this emiſſion of corpuſcles as imaginary, aſſerts,

aſſerts, that when one touches the fiſh, it is agitated itſelf with ſo violent a *tremor*, that it occaſions ſuch painful numbneſs in the hand, which touches it: *Hæc torpedo digitis compreſſa tremore adeo vehementi concutitur, ut manum contrectantis moleſto torpore dolorifico afficiat.* I do not know whether the idea raiſed in me by theſe words of *Borelli* be the ſame he intended to convey, but they gave me a notion, that a ſenſible agitation was perceived in the *torpedo*, when about to produce its effect, not unlike that undulation found in cords ſtretched horizontally, upon pulling them out of that poſition, *tremore adeo vehementi concutitur*; and he makes uſe of the ſame expreſſion a little lower, *Si tangatur piſcis eo ipſo tempore quo concutitur*; and the better to ſhew that there is a ſenſible difference between the times of its ſhaking, he adds, *Si tangatur tempore quo quieſcet.*

I watched the *torpedo* very narrowly to diſcover which of the two opinions I was to adhere to, but with all my attention I could never find, that it was in the leaſt agitated itſelf, when it ſtruck the numbneſs, and am apt therefore to imagine, that *Borelli*'s fiſh being leſs at its eaſe than mine, might on that account give itſelf certain motions to which that celebrated author was led to attribute all its force.

After having well obſerved the animal, I came at length to learn the preciſe moment when it was ready to produce the numbneſs, and was able to foretell it infallibly to thoſe who touched it, at which time I imagined I had found out the whole myſtery, whereon its virtue depends. The *torpedo*, like other flat fiſhes, is not perfectly flat, but its back, or rather all the upper part of its body is a little convex. Now I obſerved, that

while it did not, or would not produce any numbneſs in thoſe who touched it, the back retained its natural convexity, but that when it was diſpoſed to act, it inſenſibly leſſened the convexity of its back parts, bringing them ſometimes to a perfect flatneſs, and at other times rendering them inſtead of convex concave. Now was the moment come for the numbneſs to take place, the blow being ready to go forth, the arm hereupon was inſtantly benumbed, the fingers which thruſt the fiſh were obliged to let go, and all the part of the animal which had been flatted, became convex again; but whereas it had been flatted by inſenſible degrees, it turned convex again ſo ſuddenly, that one could not perceive the tranſition from the one ſtate to the other. A muſquet-ball hardly flies quicker than the fleſh does to its firſt ſituation; at leaſt the one is equally perceivable with the other. From this ſo ſudden a ſtroke ariſes the numbneſs which ſeizes the arm; accordingly the perſon who begins to feel it imagines, that his fingers have been ſtrongly ſtruck, tho' he did not ſee the ſtroke given. But in all this time, ſo far is the *torpedo* from being under the violent *tremor Borelli* mentions, that the ſmalleſt motion is not perceived on the ſurface of its body; it is evident a motion there has been, by reaſon the flatted parts are become convex again, but it is a motion ſo very quick as to eſcape the beſt eyes.

It is the velocity alone of this ſtroke that produces the numbneſs; nor is there any difficulty in ſhewing that the emiſſion of torporifick corpuſcles has no ſhare therein, but ere we proceed to that, let us conſider the wonderful compoſition of parts, or ſprings, which nature has employed to produce it; ſo ſwift a ſtroke given by a hard matter, would have broke the bodies which touched the

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orpedo, and whether this would have been too much, or whether the ſtructure of the fiſh did not allow of ſpringy hard parts, we find ſoft ones deſtined for this purpoſe, but a ſingle ſtroke of a ſoft body would not have been of force enough; and therefore a prodigious number of ſuch ſtrokes are here given at the ſame inſtant; to unfold the wonderful mechaniſm hereof we muſt explain the parts whereon it depends.

Theſe parts are two very extraordinary muſcles, called by *Redi*, and after him by *Lorenzini*, the *muſculi falcati*, here repreſented by AAAA, fig. 2 and DDDDEEEE, fig. 3, they are ſhaped ſomewhat like creſcents, and together poſſeſs $\frac{1}{2}$ the bulk of the body, being one of them on the right ſide, the other on the left, and their thickneſs that of the whole *torpedo*; they ariſe a little above the mouth and eyes, and are ſeparated from each other by the *Bronchia*, which have ten apertures, outwards ranged in two lines, as is well obſerved by *Ray*, and not in four as mentioned by *Redi*; on the oppoſite ſide to the *Bronchia* each muſcle follows the cartilage II fig. 3, to which the fleſhy fibres of the fiſh are faſtened; and theſe cartilages being a little arched, the muſcles have a like bending on thoſe two ſides, and terminate in the laſt of the *Bronchia*.

But what is moſt remarkable in them is their fibres, if we may give that name with the authors above cited, to a kind of muſcles as thick as gooſe-quills, repreſented here by MM fig. 4, of an aſſemblage whereof the two large muſcles juſt mentioned are formed, but whatever name we give them theſe little muſcles or theſe large fibres are cylinders, of the thickneſs already ſpecified, and in length nearly equal to the thickneſs of the fiſh; being all ranged aſide of each other, and perpendicular

cular to the upper and lower ſurfaces of the fiſh, if we may conſider thoſe two ſurfaces as two plains nearly parallel.

The ſtructure of each of theſe cylinders deſerves to be known; their external ſurface conſiſts of white fibres, whoſe direction is the ſame with that of the length of the cylinder, but theſe fibres only form a hollow tube whoſe ſides are no thicker than paper, all the inſide being filled with a ſoft matter of the colour and conſiſtence of pap.

This pappy ſubſtance however that fills the cavities of ſo many tubes, makes but one maſs, which is divided into 25 or 30 ſeveral parts, by ſo many little partitions parallel to the baſe of the cylinder, and theſe partitions are formed of tranſverſe fibres, ſo that the whole cylinder is compoſed as it were of 25 or 30 ſmaller cylinders placed one upon another, and each of theſe ſmaller cylinders is a cell filled with the pappy ſubſtance abovementioned.

S. *Lorenzini*, who agrees with us in giving the little muſcles a cylindrical figure, alledges however that their extremities are not round but figured irregularly, and that the marks of their irregularity appear on the skin of the fiſh, but I have always found their extremities circular; 'tis true when the *torpedo* is dried there appear irregular figures impreſſed on its skin, over againſt the two large muſcles compoſed of theſe cylindrical fibres: but theſe figures are not thoſe of the ends of the cylinders, but are impreſſed by the maſhes of two pieces of net work DDDD fig. 3, one whereof covers the muſcles on the back ſide, and the other on the ſide of the belly; the maſhes of theſe nets are irregularly figured, and being preſſed againſt the skin by the cylinders underneath them, leave impreſſions thereon which are not circular.

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Let it be remembered now that when the *torpedo* prepares to ſtrike a numbneſs it inſenſibly flattens, the external ſurface of its body changing it from convex to concave, and the whole mechaniſm whereon its force depends, will appear, for by this ſlow contraction, it ſtrains all its ſprings, makes all the cylinders ſhorter, and at the ſame time augments their baſes, that is, ſtretches the little partitions which divide the pappy matter, and perhaps the large fibres may on this occaſion depart a little from their cylindrical figure, to fill up the vacuities naturally left between them. The contraction being carried to a certain pitch all the ſprings bend, the longitudinal fibres lengthen, the tranſverſe ones or thoſe which form the partitions ſhorten, and each partition being pulled by the longitudinal fibres, in thus lengthening thruſt their pappy contents upwards, which may perhaps be promoted by the undulatory motion of the tranſverſe fibres when contracting; if now a finger happen to touch the *torpedo*, it receives a ſtroke or rather ſeveral ſucceſſive ſtroaks from each of the cylinders it is applied on. For the pappy matter being diſtributed among ſeveral partitions, tis probable the ſtrokes are not all given at the ſame time; nay, though there were not partitions to ſeparate this matter yet the ſtrokes given by it would be in ſome meaſure ſucceſſive, ſince all the parts of ſoft bodies do not ſtrike at once, ſome of them not coming in turn till others have ceaſed to act. But theſe ſeveral partitions ſerve likewiſe to augment the number of ſprings, and conſequently the velocity and force of the ſtroke.

Theſe quick repeated ſtrokes, given by a ſoft matter, ſhakes the nerves, which hereby change or ſuſpend the courſe of the animal ſpirits, or ſome equivalent fluid: or if you had rather, theſe ſtrokes

produce an undulatory motion in the nerves, which disagrees with that we are to give them in order to move the arm, hence the impotency we are under of using it, and the sensation of pain.

From this account it seems to follow that the *torpedo* is only able to strike numbness when touched over the two large muscles, composed of the cylindrical fibres abovementioned; accordingly I have found, and *Borelli*, *Redi*, and *Lorenzini* have found the same, that 'tis on these muscles the greatest numbnesses are given, the farther you touch from them, still the less danger you are in, and about the tail all is safe, which the fishermen are very sensible of, for they catch them by it, not but one may find a feeble numbness at some distance from those muscles, but it never reaches as far as the elbow, and the whole is compatible enough with our account; for the skin of the fish must feel the stroke of the muscles, and receiving a shake must communicate the same to such bodies as touch it.

Those authors who ascribe the virtue of the *torpedo* to torporifick corpuscles have been forced nevertheless to have recourse to these two muscles, to help out their account; for they consider them as reservoirs wherein the corpuscles that are to give the numbness are lodged. M. *Lorenzini*, who had observed their contraction, pretends that its use is to emit these little bodies from within the muscles, where they are imprisoned: but this emission of corpuscles, asserted by the generality of authors is overturned by the following experiments.

1st. If the hand or arm be ever so little distant from the *torpedo*, they feel no numbness, as M. *Lorenzini* himself allows. Now to use his own comparison, if the torporifick corpuscles benumb, as fiery

fiery particles heat, a hand at ſome diſtance from the fiſh muſt feel a numbneſs,as a hand at a diſtance from the fire feels warmth.

2dly, If this numbneſs was occaſioned by torporifick corpuſcules, expreſſed by the contraction of the muſcles abovementioned, the numbneſs would happen while the parts of the fiſh are contracted, whereas it does not commence 'till the contraction ceaſes. I have known this contraction continue a good while without feeling any effect from it, tho' my hand was on it all the time.

3dly. If the numbneſs were the effect of torporifick corpuſcles it would come on by degrees as the hand heats by degrees, and be at firſt weak, and at length become violent in proportion as the corpuſcles inſinuated into it. Whereas on the contrary we find the numbneſs ſtrongeſt when it begins, as all pains are, which are owing to ſudden ſtrokes,and it continues dwindling from the time it is firſt felt, being ſo violent at firſt as to force one to quit the touch.

I have ſeveral times uſed my utmoſt endeavour not to let go my hold, but all is in vain, the hand opening of itſelf as ſoon as the numbneſs began, tho' I ſqueezed the fiſh between two fingers, one of them above, and the other underneath it, in which caſe it ſeems as if it ſhould have been diſabled from acting, yet I was not then ſecure. For the finger next to that which ſqueezed it moſt had but little effect, and the next to that barely touched it, and conſequently were expoſed to the ſtroke, yet I always found the numbneſs weaker and attended with leſs pain, when I thus held the animal ſtrongly ſqueezed in my fingers, than when I put the tips of my fingers thereon.

It may be added that tho' we have ſtrongly ſpoken of a numbneſs of the arm, yet it can produce the

the like in other parts of the body, accordingly it benumbs the legs when one walks on it barefooted; not that I have experienced ſo much, for the weather was too cold, and my *torpedo* would not act out of water; but the fiſhermen unanimouſly affirm it, and add that the ſtroke ſometimes throws them down.

As to what is commonly reported, that it can ſtrike a numbneſs even when only touched with a long ſtaff, I can neither aſſert nor deny it, yet upon touching a *torpedo* with a common cane, I fancied I felt the ſtroke, tho' very ſlightly; perhaps there may be ſome ſtrong enough to make it felt at the end of a long ſtaff, but it is ſcarce credible, which M. *Perrault* mentions, that they ſhould benumb the hands of the fiſhermen who only hold the nets they are taken in.

The reaſon is obvious why its action ſhould ſcarce, or not at all be felt when one touches it with a ſtick, for the numbneſs ariſes from the extreme velocity of the ſtroke, now by the laws of percuſſion, the velocity wherewith a cane or ſtick ſome feet long ſtrikes the hand, is much leſs than that wherewith the *torpedo* ſtruck the ſtick, and conſequently a leſs effect muſt ariſe from it; for the ſame reaſon the longer a cane is the leſs it will act on the hand, but a thin body being put between the finger and the *torpedo*, the diminution of the velocity is not ſo great but that the numbneſs is conſiderable; hence we have a new proof that the emiſſion of torporifick corpuſcles contributes nothing to the numbneſs. For the finger when only a line diſtant from the fiſh receives no impreſſion therefrom, if the intermediate ſpace be only filled with a fluid ſubſtance as air or water, but if this ſpace be poſſeſs'd by a ſolid body, the *torpedo* then makes an impreſſion on the finger; now whence

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does this arise, unless it be that the solid body communicates the impression it had received. It would have been worth knowing what the thickness of the body must be, whose interposition between the *torpedo* and the finger indemnifies it therefrom, but as the animal acts weaker or stronger at pleasure, I have not been able to determine it.

This faculty of benumbing would be of no great advantage to the fish, if the only use it made of it was to defend itself from the fishermen, for it rarely happens to escape their hands. But *Aristotle*, *Pliny*, and the generality of naturalists likewise affirm that it serves it to catch fish withal, all we know of the matter is that it feeds on fish, which is frequently found in its stomach, and yet the *torpedo* like most flat fishes, usually keeps on the sand or mud, where it may perhaps lie on the scout. 'Tis certain its powers would be much to be feared by such fishes as it could touch; I had no opportunity of making experiments, but am well assured that the redoubled strokes it is able to give might kill much stronger animals than little fishes.

For want of a fish, I took one of the land animals nearest a kin thereto, *viz.* a duck, and putting the *torpedo* and it together in the same vessel of sea water, only covered a top with a linnen cloth to prevent the duck from flying away, in a few hours time I found my duck quite dead, it had doubtless touched the *torpedo* too often, which cost it its life.

If we may believe what is related of the *torpedo* in the history of *Abyssinia*, it is no less able to bring dead fishes to life again than to kill living ones; it being there asserted, that a dead fish put in the same vessel with the *torpedo* will stir about

again ; but it may be thought too much to have barely mentioned ſuch a fable, another fact related in the hiſtory of the ſame country, is that the *torpedo* is uſed to cure fevers. The method of application is as follows, the patient being tied down cloſe to a table, they apply the fiſh ſucceſſively on all his parts, which puts him indeed to a cruel torture, but certainly delivers him from his diſeaſe. *Bellonius* mentions it as a current notion that a dead *torpedo* applied to the ſoles of the feet, abates the violence of a fever.

In *America* they have *torpedo's* which reſemble ours as to their effects, but are very different from them in figure. M. *du Hamel* in the hiſtory of the academy for the year 1677, mentions a ſpecies ſhaped like eels, and therefore compared to congers, M. *Richier*, from whom the account came, affirms that it benumbs the arm, when touched with the end of a ſtaff, and that its effects are ſo violent as to produce vertigos, which he aſſures us he had found by experience.

When the *torpedo* is dead, the fiſhermen no longer fear it, but eat it like other fiſh, tho' it makes no very delicious food; not to mention how little there is of it, for they throw away the two large muſcles, as having little in them beſide the pappy matter, whoſe taſte at beſt is but faint and ſickly.

An explanation of the figures relating to the torpedo, *in Plate* I. *Tranſlated by* J. M.

Fig. 2. repreſents a *torpedo* ſeen on the back.

The dotted line AAAA ſhews almoſt the circumference of one of the two muſcles, on which the extarordinary effect of this fiſh depends. It is the ſurface contained within the ſame line, that flattens

flattens, when the fiſh is going to numb. On the other ſide there is a muſcle of equal extent, which as well as all the other parts on this ſide, are ſhortned here by the perſpective.

Fig. 3. ſhews the belly of the fiſh, where one of the muſcles that numb is diſcovered.

B is the mouth of the fiſh.

CCCC its gills.

DDDD,EEEE is one of the two muſcles, which cauſes the numbneſs, at DDDD it is covered with a net work, at EEEE this net work is raiſed, and we ſee only the fibres of the muſcle. The muſcles would appear in the ſame manner, if they were uncovered at the back.

FFFF the skin, which covered the muſcle, raiſed up.

GG a fleſhy muſcle, or fleſh of the fiſh.

HH a bundle of veſſels, which carry a ſlimy matter, that ſpreads over the upper part of the body of the fiſh.

II a cartilage along which the preceding veſſels take their courſe.

KKKKK diverſe branches of veſſels for the viſcous matter, the apertures of which terminate in the exterior ſurface of the skin, as may be ſeen at LLL.

Fig. 4 is a maſs of the thick fibres, which compoſe the muſcle.

MM is one of theſe fibres. The circular lines parallel to their ends are *ſtrata* of fibres, which divide each of the great ones into ſeveral parts.

VIII. *A comparison of the ancient* Roman *foot with that of the* Chatelet *at* Paris, *with some remarks upon other measures*; *by M.* de la Hire*.

I gave the academy in 1701 † some remarks which I had made on the capacity of our *pinte*, according to the standard at the *Hotel de Ville* of *Paris*; and on the weight of the water which it contains; and I shall now relate some observations on the size of the ancient *Roman* foot compared with ours, such as is now in use, that is, with that of the *Chatelet* at *Paris*, which is $\frac{1}{6}$ of the toise.

Mess. *Picard* and *Auzout* had made several remarks on these very measures, which I have printed in the collection of the posthumous works of our academicians.

We must not be surprized if we find some difference in the examination which we make of what remains to us of the ancients, to conclude from it the size of the ancient *Roman* foot, for it is not long since the architects and masons at *Paris* still made use of a foot, which was greater by about a line than that which is at the *Chatelet*, with regard to the toise, which serves for a standard to all our measures.

The reformation of the mason's foot was made in 1668, at the same time that the toise of the *Chatelet* was established, as we see it now at the entrance and under the great stair-case; for the ancient toise, which is still to be seen, is applied on the outside in the court against one of the pillars of the building, but this toise is quite falsi-

* December 1 1714,

† Vol. I. page 285 of this abridgement.

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Fig. 1.

Fig. 2.


Fig. 3.


Fig. 4.

J. Mynde sc.

ſified at top, by the pillars bending in that place. I have now in my hands a very old mathematical inſtrument, which had been made by one of our moſt able workmen, with very great care, wherein the foot is marked; and it has ſerved to reſtore the toiſe of the *Chatelet*, according to what I have learned of it from our old mathematicians.

As for the ancient *Roman* foot, we ſtill ſee at *Rome* upon 2 marble ſepulchres of 2 architects, the figure of this foot in *baſſo relievo*; the extremities of it are a little worn, both by time, and by the meaſure, which all the curious have taken of it; however, tho' they are very coarſely made, one of them is ſtill 10 inches, 11 lines $\frac{1}{2}$, of our meaſure, and the other 10 inches, 11 lines $\frac{1}{1}$, whence we might judge them to have been 11 inches at firſt, for $\frac{1}{4}$ of a line either of one ſide, or the other, is very little for a ſculptor in marble. One of theſe feet is very ill divided into its 16 digits, and the other is not divided.

However the brazen ſtandard of the ancient *congius* of the time of *Veſpaſian*, which is preſerved very entire in the *Farneſian* palace at *Rome*, gives the meaſure of the ancient foot at that time, 11 of our inches, and 1 line, according to the relation of *Villalpandus*; for the *congius*, which among the *Romans* was the meaſure of the liquors, as our *pinte* is here, ought to contain $\frac{1}{8}$th of their cubical foot; but it is always very difficult to meaſure the capacity of a veſſel by means of liquors, eſpecially when the veſſel has its aperture very large, as that has.

For this reaſon I have been deſirous to ſee, whether the meaſures of the ancient foot ſtill remaining, could inform us of the ſize of the ancient *Roman* foot; and as the *Pantheon* or *Rotun-*

da is one of the moſt entire and moſt beautiful pieces of all antiquity, I have found that the height of the ſhaft of the columns of the portico, which are of oriental granite, is 36 feet, 7 inches $\frac{1}{3}$, or 439 inches $\frac{1}{3}$ of our meaſure, and ſuppoſing that the ancients intended to make theſe columns 40 of their feet; for it is probable that *Agrippa*, who decorated this temple, uſed all poſſible care, we ſhall find that the *Roman* foot was 10 inches, 11 lines $\frac{3}{4}$, of our feet: and in reducing in like manner the diameter of theſe columns at the baſe, we ſhall find that there are 5 *Roman* feet, of the ſame meaſure, and a little more, but that they are not all exactly of the ſame thickneſs, which may be aſcribed to the great difficulty of working the *Egyptian* granite of which they are made, becauſe of its hardneſs.

The breadth of the opening of the gate of this temple is 220 inches $\frac{1}{3}$ of our meaſure, and ſuppoſing it to be 20 *Roman* feet, we ſhall find each of them to be 11 inches, and $\frac{1}{10}$th of a line. We can conclude nothing certain from the height of this gate, becauſe the architect was obliged to conform to the height of the portico.

The length of the portico of this temple, meaſuring it from outſide to outſide of the walls, muſt have been 110 *Roman* feet, reckoning them at 11 of our inches.

If we would reſt here, we ſee that taking a mean between the meaſures juſt related, we muſt reckon the antient *Roman* foot to be 11 of our inches.

But if we carry this examination farther, we ſhall find that the round temple of *Bacchus* muſt be 75 *Roman* feet in diameter, ſuppoſing each of them 11 inches, and the breadth between the exterior

terior face of the wall, and the columns is 25 feet, which is $\frac{1}{3}$ of the whole diameter.

The temple of *Faunus*, which is alſo of a circular figure, has its diameter exactly of two hundred and ſixteen feet, of eleven inches each, of our meaſure.

If we ſuppoſe, that the breadth of the gate of the temple of *Veſta* at *Tivoli*, was 10 ancient feet; this foot will be 10 inches, 11 lines, $\frac{5}{8}$, of our meaſure, and ſuppoſing the columns of this very temple to be 2 feet $\frac{1}{2}$, in diameter, the ancient foot will be 11 inches $\frac{2}{5}$ of a line.

If the columns of the temple of *Antoninus* and *Fauſtina*, were 5 ancient feet in diameter, the foot muſt have been 10 of our inches, and 11 lines.

Theſe laſt obſervations concur with the firſt to ſhew that the ancient *Roman* foot was 11 inches of our foot.

We know that the ancient *Roman* foot was to the *Greek*, as 24 to 25; for the *Roman ſtadium* was to the *Greek* as 600 to 625 *Roman* feet, and conſequently the *Greek* foot muſt have been 11 inches and $\frac{1}{2}$ of our meaſure, or thereabouts.

As for the modern meaſures, the *Roman* palm at preſent contains 8 inches $\frac{1}{4}$ of our foot, but the palm among the ancients was $\frac{3}{4}$ of the foot; wherefore the foot muſt have been 11 inches, if this palm was formed upon the ancient foot, as is very probable. The *canna* of the architects contains 10 palms, wherefore it is 6 feet, 10 inches $\frac{1}{2}$ of our meaſure. But the *canna* of the merchants at *Rome* is only 6 feet, 1 inch $\frac{1}{2}$ of our meaſure; they divide it into 8 palms, which are greater than the palm of the architects, the ſixth part of this *canna* would be very little different from our foot.

The

The meaſures of the *aulne* made uſe of at *Paris* are different among different trades, for ſome are ſhorter than others by about 4 lines. I have found in the hands of one of our mathematical inſtrument-makers, a great brazen rule which was ancient, and ſaid by him to be the meaſure of the *aulne*; it was exactly 44 of our inches. But there was printed at *Paris* ſome time ago, a treatiſe of commerce, the author of which is much eſteemed, and he relates, that the mercers *aulne* is 44 inches wanting 4 lines, according to the ſtandard preſerved in their *bureau*. I was deſirous to ſee myſelf what it was, and having meaſured this *aulne* very exactly, I found it was 44 inches wanting only ½ a line. It is eaſy to meaſure it, for this ſtandard is a thick iron rule, with two pieces of iron faſtened perpendicularly at the ends, between which the rule may be applied that we would meaſure: at the back of this rule it is engraven in great capital letters, that this is the *aulne* of the mercers, and wholeſale dealers, (*groſſiers*) 1554.

We ſee that this *aulne* comes very near 4 ancient *Roman* feet, as we determined before. We might therefore ſuppoſe that when the *Romans* were maſters of theſe countries, they introduced their meaſure for the merchants, and made the *aulne* 4 of their feet, which is a little altered among different tradeſmen; for the woollen-drapers make it a little leſs than the mercers, and the linnen-drapers make it four lines ſhort of 44 inches, which is the leaſt that is in uſe.

I ſhall here add an obſervation which I have made on the word *demi-ſextier*, which I believe came to us from the *Romans*, as well as the meaſure of liquids very nearly. For this word *demi-ſextier* has no relation to our *pinte*, ſince the *chopine*

pine ſhould be the *ſextier*, which is not the ſixth part of our meaſures; but the *ſextarius* of the *Romans* was the ſixth part of their *congius*, which was the eighth part of their cubical foot, and their *demi-ſextarius*, which they alſo called *hemina*, is almoſt equal to our *demi-ſextier*, which ſhould contain 12 of our cubical inches, the *pinte* containing 48 according to the common meaſure.

If the ancient *Romans* had left us in writing the exact meaſure of ſome parts of their principal edifices, after they were perfect, we ſhould have at preſent the juſt ſize of their foot, by comparing thoſe meaſures with our own, which we can only do by conjecture; wherefore I ſhall here relate the exact dimenſions of ſome parts of our principal buildings, that we may have recourſe to them, if the meaſure we now make uſe of ſhould change, or be altered by any accident.

I have meaſured exactly with our toiſe and foot in the building of the obſervatory, even with the ground, the breadth of the north-door between its poſts and above the threſhold; and I found it to be 8 feet, 0 inches, 8 lines; and in like manner the breadth of the ſouth door, which opens upon the terraſs, is 7 feet, 11 inches, 8 lines.

The length of the great hall of the ſame building between the walls, from inſide to inſide, is 15 toiſes, 3 feet, 0 inches, 6 lines; and its breadth in like manner between the walls from inſide to inſide, without reckoning the hollows, and over-againſt the door which opens to the great ſtair-caſe, is 7 toiſes, 5 feet, 2 inches.

At the church of *Val de Grace*, in the *Faux-bourg Saint Jacques*, the breadth of the great door between the poſts at the bottom upon the threſhold is 9 feet, 9 inches, 3 lines. The breadth of the body of the pilaſters of the portal, which

are at the ſides of the door on the outſide, is 3 feet, 3 inches. The breadth of the nave of this church at the entrance between the *ſoques* placed under the firſt pilaſters, and meaſured upon the pavement of the church, is 4 toiſes, 5 feet, 4 inches, 3 lines.

At the *Louvre* in the court, the breadth of the ſquare door which is toward *Saint Germain de l'Auxerrois*, or toward the eaſt, taken between its poſts and upon the threſhold, is 12 feet, 0 inches, 6 lines.

The breadth of the other ſquare door, which is toward the *Thuileries*, or toward the weſt, meaſured in like manner between its poſts, and upon the threſhold, is 10 feet, 2 inches, 6 lines.

The manner of meaſuring magnitudes exactly, is to have 2 toiſes very exact, and ſhod with iron at the ends, one of which at leaſt ſhould be divided into feet and inches, and to apply them in a right line upon the place that we would meaſure, placing the end of one againſt that of the other, and obſerving not to remove one till the other is placed, for if we pretend to make theſe meaſures only with one toiſe or foot rule, marking where its extremity ends, we ſhall continually fall into ſome miſtake.

A

A
TABLE
OF THE
PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXV.

In the HISTORY.

In the MEMOIRS.

A N

AN ABRIDGMENT OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1715.

I. *Of a dog endued with the faculty of ſpeaking.*

WITHOUT ſuch a teſtimony as that of M. *Leibnitz*, an eye-witneſs, we ſhould not have ventured to have related, that near *Zeitz*, in *Miſnia*, there is a dog which ſpeaks. It is a peaſant's dog, of the moſt common figure, and a middling ſize. A little boy heard him make ſome ſounds, which he thought reſembled ſome *German* words, and upon that he took it into his head to teach him to ſpeak. The maſter who had nothing better to do, did not ſpare either his time or his pains; and happily the diſciple had ſuch diſpoſitions that would have been difficult to be found in any other. In ſhort, in a few years the dog knew how to pronounce about thirty words. Of which number are, *Thé*, *Coffé*, *Chocolat*, *Aſſemblée*, *French* words, which have paſſed into the *German* without alteration. It muſt be obſerved that the dog was full three years old when he was put to ſchool. He only ſpeaks by echo, that is to ſay, after his maſter has pronounced a word; and he ſeems to repeat it then by force, and againſt his will, tho' they do not uſe

him ill. M. *Leibnitz* ſaw and heard him another time.

II. *Of the ſudden fall of a mountain.*

M. *Scheuchzer* informed the academy, that in *June* 1714, the weſtern part of the mountain of *Diableret*, in *Valais*, fell ſuddenly, and all at once, between two and three in the afternoon, the heavens being very ſerene. It was of a conical figure. It threw down 55 cottages, cruſhed 15 perſons, and above 100 kine, and many more ſmall cattle, and covered with its ruins a full league ſquare. There was a profound darkneſs cauſed by the duſt. The heaps of ſtones amaſſed below are above 30 perches high, which are probably the *Rhinland* perches of 10 feet. Theſe heaps have ſtopped the waters, which form new and very deep lakes. In all this there was not any ſigns of bituminous matter, nor ſulphur, nor lime, and conſequently no ſubterraneous fire. Probably the baſe of this great rock was rotted of itſelf, and reduced to powder.

III. *Of the ſea-hare, or ſea-cat.*

The ſea-hare or cat is an animal, which, notwithſtanding its name, walks very ſlowly, and has no legs. It reſembles the land-ſlug, and has horns like that, but flat. M. *de Reaumur* has obſerved on the coaſt of *Poitou*, the manner in which the male and female of this ſpecies couple. The female has the aperture of the feminine part almoſt in the middle of the back. The male mounts her, and there comes out from beneath his belly a maſculine part turned ſpirally, almoſt like that of drakes.

IV. *Of a monſtrous lamb.*

M. *de Puy*, the king's phyſician at *Rochefort* writ to M. *de Lagni*, that he had ſeen a monſtrous lamb come at its full time, but which muſt die the moment it was born, becauſe it had only one little hole placed between its two ears, by which it could receive a little air, and that this hole had no entrance into the lungs, but only into the *œſophagus*; and this canal was alſo ſwoln with air, and quite blown up. This hole was the only throat of the animal, and it was abſolutely impoſſible for any nouriſhment to paſs that way. The lamb was therefore only nouriſhed from the navel-ſtring. It is proved by a great many other examples, that the navel-ſtring without a mouth is ſufficient. Both the ſtomachs of the lamb were full of ſlime, like the white of an egg, and the inteſtines full of *meconium*. If it be true, as ſome authors relate, that they have ſeen *fœtus's* very well nouriſhed without *placenta*, and without *umbilical* veſſels, the mouth and the navel are equally paſſages for the nouriſhment of the *fœtus*, and they ſupply one the other ſo perfectly, that it is not perceived that one of the two is wanting; but if this fact is not true, the navel is the only paſſage.

This lamb had the hair of a wolf or dog. Probably ſome great fright of the mother had been the cauſe of it, and at the ſame time produced the other diſorders which rendered this *fœtus* monſtrous.

V.

V. *Of a contagious diſeaſe.*

There reigned for ſome time in the villages about *Toul*, a contagious diſeaſe, by which they were carried off in two or three days at moſt. It was ſo violent a purple, that the ſkin entirely came off from thoſe who eſcaped. The infection of the carcaſſes was ſo great that nobody would open them, and ſeveral died upon carrying them to their interment. It was very remarkable in this diſeaſe, that thoſe who were ſoon relieved, voided worms, after which the purple appeared. This account was ſent to the academy, by M. *Geoffroy* the younger brother.

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1715.

I. *Meteorological observations during the year* 1714, *at the royal observatory; by M.* de la Hire*.

I Observed the exact quantity of water which fell in rain and melted snow during the last year 1714, making use of the same instruments, and in the same manner as in the preceeding years; I found that the height of water had been during the months of

	Lin.		*Lin.*
Jan.	$4\ \frac{1}{2}\ \frac{1}{4}$	July	$28\ \frac{1}{4}\ \frac{1}{8}$
Feb.	$9\ \frac{1}{4}$	Aug.	$9\ \frac{1}{2}\ \frac{1}{4}\ \frac{1}{8}$
March	$11\ \frac{1}{4}\ \frac{1}{8}$	Sept.	$22\ \frac{1}{2}\ \frac{1}{4}$
April	$5\ \frac{1}{2}\ \frac{1}{4}\ \frac{1}{8}$	Oct.	$17\ \frac{1}{8}$
May	$16\ \frac{1}{2}\ \frac{1}{8}$	Novem.	$0\ \frac{1}{4}\ \frac{1}{8}$
June	30	Dec.	$20\ \frac{1}{2}\ \frac{1}{4}$

The sum of the height of water of this whole year is therefore 177 lines $\frac{1}{8}$, or 14 inches 9 lines $\frac{1}{8}$, which is very far from 19 inches, at which we have estimated the water which falls in each mean year. We may therefore say that this year has been very dry, for the three months of *June July* and *August*, have only furnished about six

* Jan. 9, 1715.

inches

inches; and they very often produce as much as all the reſt of the year; but this is commonly by ſtorms, and theſe ſort of rains are of no ſervice in fertilizing the earth, becauſe they run off almoſt as ſoon as they fall, and do not penetrate into the earth. I am perſuaded the fogs that are formed by vapours and exhalations are much more uſeful for the nouriſhment of plants than the rains: as there have been a great many very thick fogs during this year, the harveſt has been very plentiful, and the fruits have been very well ripened. It is alſo obſerved that in theſe countries, where the greateſt part of the grounds are pretty damp, the dry years are more proper for the fruits of the earth, than the rainy years are.

My thermometer which is at 48 parts in the bottom of the caves of the obſervatory, where it remains in all weathers at the ſame height, fell at the loweſt the 5th of *February* to 20 $\frac{1}{2}$, which does not mark a great cold, for it pretty often falls to 14 parts, and preſently after it roſe conſiderably. There fell but very little ſnow as well at the begining as at the latter end of the year. This thermometer roſe to 64 parts *July* 10th at ſun-riſe, which is the coldeſt time in the day, but at $2^h \frac{1}{2}$ P. M. the ſame day it roſe to 74 parts, which may be looked upon as the meaſure of the greateſt heat of this ſeaſon; ſo that the greateſt heat of this year has only ſurpaſſed the mean ſtate, about as much as this mean ſtate has ſurpaſſed the greateſt cold which commonly happens.

There have been violent winds ſeveral times this year, but they have not done any miſchief in compariſon to that which happened on the coaſts of *England* and *Flanders*. There have been a few ſtorms, and ſome thunder, which commonly happens

pens in ſummer, the river has alſo been very low all this ſeaſon.

The winds have been very variable this year, but the north wind has prevailed.

The barometer which I uſe for my obſervations is always placed at the top of the great hall of the obſervatory; I found the quickſilver at the higheſt at 28 inches 5 lines, *December* 7, at a calm and foggy time; and it was at the loweſt at 27 inches 1½ lines the 9th and 10th of *May*. This barometer has very often been above 28 inches, and at that time it has not rained, which almoſt always happens. I have another barometer the quickſilver of which is always 3 lines higher than in this, which I uſe for obſervations, tho' it is placed near the other. The difference between the greateſt and leaſt height of this barometer was 15½ lines, and it is commonly 18.

I found the declination of the needle at the end of *December*, to be 11 degrees 30 minutes toward the weſt, with the ſame needle of 8 inches long, and in the ſame place where I commonly obſerve it.

II. *On the pendulums that ſwing ſeconds*; *by M.* de la Hire*.

A famous clock-maker ſome time ago propoſed to me a thought which he had concerning our great pendulums which mark ſeconds of time. He ſaid that the length of the *fourchette* from its ſuſpenſion was too ſmall to make the vibrations of the pendulum equal, and that he found it better to prolong it quite to the ball, for by this means the rod of the pendulum could govern the motion of the *fourchette*, and at the ſame time the motion of

* July 24, 1715.

the ſwing wheel, and of the whole clock, and that the vibrations of the pendulum would always have the ſame extenſion, ſince the arches of the extremity of the *fourchette* long or ſhort might be the ſame; and beſides he added, that when the *fourchette* is ſhort, it gives a bend to the ſilk which ſuſtains the rod, at the place where it is faſtened, which would not happen if the *fourchette* was very long, and that this bend may cauſe ſome irregularity in the vibrations.

I anſwer in the firſt place to theſe obſervations, that if the *fourchette* is very long, it muſt have a much greater force in the ſwing wheel which carries the *fourchette*, than if it is ſhort, to entertain the motion of the pendulum, ſince this *fourchette* will then act much farther from the centre of its motion, againſt the rod of the pendulum, than when it is ſhort, and to give this force to the ſwing wheel, a very great weight muſt be put for the principle of motion of the whole clock, which will alter all the parts of it in a very little time; for it is not the pendulum that has a motion of itſelf to entertain that of the clock, but the clock muſt entertain the motion of the pendulum, which at the ſame time regulates the motion of the *fourchette*, which can never be equal, and the weight of the ball of the pendulum will have much more action againſt the ſhort *fourchette*, than againſt the long one, ſince the motion of both of them have their common *axis*.

For this reaſon the length of the *fourchette* has been the leaſt that could be, to make the clock move with a leſs weight.

In the ſecond place, the bend of the ſilk, which ſuſtains the rod of the pendulum, is of no conſideration, for the rod of the pendulum has a free motion in the ſlit of the *fourchette*, which is always

done, therefore this consideration is of no weight. For if we would take away the silk which sustains the rod of the pendulum, and put a spring in its place, which should be pinched between the extremities of the cycloid, I do not know whether the cycloid could then have the same effect upon this spring as upon the silk; but I know very well that the experiment which I formerly made of a likespring to sustain the rod of the pendulum informed me that it caused in the motion of the clock very great inequalities, which I could ascribe only to the spring, which becomes more stiff or flexible, according to the different constitutions of the air, which hindred me from being able to regulate my clock, which I did as soon as I had taken this spring away, and put a silk in the room of it. We must not pretend however, that this silk can have its effect at first, as we observe when we put a new one, for it must have some days to acquire all the extension of which it is capable, which must determine the length of the pendulum, and consequently the duration of its vibrations, whereon the regularity of the clock depends.

III. *On the mines of turquois-stones in* France; *on the nature of the matter found there, and the manner in which they are coloured, by M.* de Reaumur*; *translated by Mr.* Chambers.

France is not very rich in mines of precious stones, its soil is rather fruitful in productions whose value has no dependance upon fancy and opinion, and yet is not altogether destitute of those rare stones, which by an almost unanimous

* Nov. 13, 1715.

consent, are set at so high a price. Our failing is, that we over-look our own riches. *Persia* is famous among us for its *turquoises*, and we even envy it, tho' the mines of that stone are less frequent in *Persia* than in our own country; nor are the *French* turquoises much inferior to those brought from the east, but in some respects more worthy the attention of the lovers of natural knowledge.——This shall be shewn at large when after examining turquoises in the general, we come to a particular *examen* of those of our own growth.

The turquois is esteemed the first among opaque stones; its colour is blue, which in those most valued, is neither deep nor light, and is above all things to be free of any cast of whiteness, that is, as the jewellers express it, it must not resemble starch-blue, but the blue rather of verdigrease in the lump, yet without any sensible cast of green, tho' it may border a little upon the greenish.

'Tis one of the softest among precious stones, its hardness being scarce equal to that of a crystal, or a transparent flint, but some of them are much softer than others, the hardest *cæteris paribus*, are the most beautiful, and this by reason of the vivacity of the polish, which in all stones is proportionable to their hardness; those whose colour is fine, and their polish or lustre sprightly, without any threads, specks, or inequalities on their surface, and which weigh several carats, are highly valued. *Rosnel*, a jeweller, in his treatise of precious stones, entitled, *Mercure Indien*, coming to rate the several stones, put turquoises with the qualities above-mentioned, on the footing of the most perfect emeraulds, that is with diamonds themselves. Indeed there are few such found of any considerable bulk without failings, which make a considerable diminution in their value. The

same

ſame *Roſnel* who rates the perfect ones ſo high, only values the ſmaller ones which have any other fault at a crown the carat.

The firſt turquoiſes ſeen in *Europe* probably came from *Turky*, and hence might their name ariſe, tho' ſome etymologiſts fetch it much further. 'Tis not eaſy to determine by what name it was called among the ancients: the characters they give of moſt ſtones are ſuch as frequently make it impoſſible to know them, and the procedure of ſeveral of the moderns will make the caſe much the ſame with poſterity. How will they be puzzled to know what the ſtone is, which we call turquois, when they read in the book of *Burquen*, a jeweller by profeſſion, that this ſtone is tranſparent, and only derives its opacity from the *collet* it is ſet in, for it is certainly opaque if any ſtone be ſo. I have broke ſeveral of them to get thin pieces, and upon viewing ſome of them, ſcarce half a line thick, againſt the ſun, could never perceive any ſign of tranſparency.

Some take this ſtone for that called by *Pliny borea*, which he ranges among the ſpecies of jaſper; others will have it, that which he calls *Calais*, tho' he ſays expreſly, that this laſt ſtone is green. *Roſnel* relates the manner of drawing turquoiſes out of the mines after the hiſtory, or rather the fable, given by *Pliny*, of the manner of procuring the *Calais* of *Sienna*; he aſſerts, that this ſtone is only found on the tops of ſome rocks, made unacceſſable by ice, and that they beat them down by ſlinging ſtones at them, whence it is that ſo few of them are found whole. Theſe rocks muſt doubtleſs be placed very favourably, that in ſpite of their ice, the *Calais* or turquoiſes ſhould fall where they may be taken up. Several other dubious things have alſo been related as to the country

country wherein the turquoiſes are found; ſome have aſſerted that the fineſt come from *India.* *Boetius* adds, that *Spain* produces them as well as *Germany*, and that they are found plentifully in *Bohemia* and *Sileſia.* *Tavernier*, who was led by his commerce to inform himſelf of precious ſtones, and who never ſpared his ſteps, affirms, that there are only two mines of turquoiſes known in the eaſt, and that both of them are in *Perſia.* "One of "them, ſays he, called the old rock within three "days journey north-weſt of *Meched*, near a large "town called *Necabourg*; the other which is "called the new rock, is 5 days journey off. "Theſe latter have but a poor water as border-"ing too much on white; and therefore but lit-"tle ſet by, and ſold almoſt for nothing. But "the king of *Perſia* for ſeveral years paſt has for-"bid digging in the old rock for any perſon but "himſelf; the art of enameling on gold being "unknown in that country, he reſerves theſe "ſtones for the enriching of ſabres, ponyards, "and the like, in order whereto the turquoiſes "are cut and applied in *collets*, ſo as to form a "ſort of flowers and other figures, which indeed "ſtrike the eye, and are the effect of infinite pa-"tience, but without any fancy in deſigning."

'Tis probable the old rock is now exhauſted, or at leaſt that the ſtones are much rarer than in *Tavernier*'s time. In the embaſſy lately ſent by the *Sophi* of *Perſia* to *Lewis* XIV. part of the preſents conſiſted in turquoiſes, which yet were all of the new rock, as appears by their colours bordering on white, like thoſe mentioned by *Tavernier*; beſides that they do not admit of a full poliſh, and their ſize being but ſmall.——In effect it might not be difficult for us to ſend finer and larger

larger turquoiſes to *Perſia*, if we would be at the pains of digging our own mines.

Jewellers and lapidaries divide turquoiſes, as they do all precious ſtones, into oriental and occidental, and ſometimes into thoſe of the old and new rock; this diviſion has helped to keep our ſtones under, for they give the eaſt, or the old rock, the credit of all the perfect ones, aſcribing only thoſe which are of ſmall value, to the weſt or new rock, ſo that if our mines produce fine turquoiſes, they preſently commence oriental. I gave an able lapidary ſeveral turquoiſes to cut, which all came out of our own mines, being deſirous to know what degree of hardneſs they had, what poliſh they would take on the wheel, and what their colour would be when poliſhed, the lapidary found a great difference between them, and in proportion as he cut them, ſhewed me which were of the old, and which were of the new rock. Among the former there was one, which tho' very ſmall, yielded nothing in hardneſs to any of the ſtones of this kind its poliſh of conſequencewas very lively, and its colour perfectly fine. I repreſented to my lapidary, that theſe different pieces all came from the ſame mine, the fact he did not conteſt, but neither did he change his language, and this becauſe a fine ſtone, and a ſtone of the old rock, are with them ſynonymous terms. Yet the effect of this way of ſpeaking is to make it believed, that the ſtones of our growth are of no value, and and that our mines are not worth working.

The mines in this kingdom which yield turquoiſes are in the lower *Languedoc*, near the city *Simore* and the country adjacent; as at *Baillabatz* and *Laymont*, as alſo towards *Auch*, *Gimont*, and *Craſtes*. *Borelli* in his antiquities and rarities about *Caſtres*, mentions their being alſo found at *Venés*, but

but M. *de Basville*, intendant of *Languedoc*, could notwithstanding all his pains meet with any thing like them, nor is it even known at *Venés*, that ever there had been any. It is alfo forgot at *Simore*, when and by what accident the turquoife mines there were difcovered, only it is a tradition that they have been known upwards of fourfcore years; the oldeft author that feems to mention them is *Guy de la Broffe*, in his treatife upon the nature, virtue and ufefulnefs of plants, printed in 1628; tho' his words are but fhort and need a commentary. Mentioning the mineral unicorn, he refers us to a marginal note, wherein he adds, "that "this unicorn is a ftone fhaped like a horn, which "being laid gradually on the fire affords the true "turquoife, it is called the mineral unicorn from "its refembling the horn of an animal." In another place he calls the mineral unicorn the *mother of turquoifes*. Now it being no common property of all mineral unicorns to affume the colour of turquoifes, it is probable the author here fpeaks of the turquoifes of *Simore*——Be this as it will, all the *French* authors upon precious ftones, I have feen, fpeak very negligently of our turquoifes, and omit what makes one of the fineft articles in the natural hiftory of this kingdom, they juft mention them under the name of turquoifes of the new rock, without entering into any detail of the nature of the matter whereof they confift, the manner of digging this matter out of the mine, and of giving it that beautiful colour, which are the three articles we here principally propofe to examine.

It muft not however be omitted, that *Berquen* mentions thefe turquoifes as procured in the lower *Languedoc*, from a whitifh rock, which by heating in the fire comes to this blue colour, but this is all he fays. *Boccone*, a *Sicilian* author, has wrote

wrote on them more at large, and yet owns that he learn'd all he knew of them from a clockmaker of *Lyons*. This however is not the only inſtance wherein we have been inſtructed by foreigners, as to what is moſt remarkable among us.

Being employed in deſcribing the arts relating to precious ſtones, I thought it my duty to enquire what there might be of that kind in this kingdom, but being too far from *Languedoc*, and wanting the conveniences of obſerving the turquoiſes in their own mines, the *Abbé Bignon* was pleaſed to engage M. *d'Imbercourt*, intendant of *Montauban* to ſend me ſuch pieces of the ſtones as I ſhould need, with anſwers to any difficulties or queſtions I might propoſe; this M. *d'Imbercourt* executed with great exactneſs, and thus furniſhed the firſt materials of this memoir.

Indeed it was time, more than ever, to examine theſe mines to the bottom, as being juſt ready to fall into oblivion, from whence it was not eaſy to reſcue them, it being above 20 years ſince any digging had been in them; the wars, the ſcarcity of proviſions, with the little value ſet on our own productions, had put a ſtop to the working, a complaint which now has no place, his royal highneſs the duke of *Orleans* ſoon after reading this memoir in a publick aſſembly, having given order to M. *le Gendre*, into whoſe department this country was fallen, to dig up the mines, and ſend pieces thereof to the academy; this he performed accordingly, and hence we have drawn new obſervations for the perfecting of the preſent piece.

There are ſeveral of theſe mines within the juriſdiction of *Simcre*, and the people of the place are ſatisfied that there needs only digging to diſcover many more. Chance has always a hand in the diſcovery of mines, but here it muſt likewiſe

have taught the method how to uſe them. The matter of theſe mines has nothing extraordinary in it whereby to draw attention; that beautiful blue that pleaſes us in the turquois being here hardly viſible, the prevailing colour is ſometimes white, and ſometimes like that of the *Venetian Tripoli*. Other precious ſtones come out of their mine with the ſame colour that they ſhew when cut; their colour is incapable of any addition, but may ſometimes be diminiſhed, as the ſaphire for inſtance, which from a too deep is brought down to a pale colour by fire; and by the like means, a pale ſaphire may be intirely ſtripped of colour, and brought to the water of a diamond; our turquoiſes on the contrary are naturally whitiſh or yellowiſh, not unlike the colour of a common building ſtone, but being expoſed ſome time to the action of fire, inſtead of becoming whiter they take a blue colour, ——an effect which it was impoſſible to have foreſeen——But ere we come to the method of colouring this matter, it may be proper to enquire a little further into its nature and origin.

It will be thought very extraordinary that we ſhould owe one of our ſpecies of precious ſtones, to the great renverſions of the ſurface of the earth, and that this ſtone ſhould anciently have been a bony matter, and yet all thoſe who take the regular figure of ſeveral ſtony matters, is an indication of what thoſe matters originally were, I mean all thoſe who conſider ſtones in the figure of ſhells, as ſo many petrified ſhells, and for inſtance the *gloſſopetræ*, &c. for the teeth of fiſhes or other animals, the retainers I ſay to this probable and generally received doctrine, will hardly doubt but that the matter of our turquoiſes is bones petrified, for moſt of the pieces procured out of the mines have the figures thereof.

It

It is the ſtanding tradition of the country, that ſome of them reſemble bones of legs, others thoſe of arms, and others teeth. No great dependance it is true ought to be placed upon reſemblances of figure, which are rarely examined with ſufficient rigour, but there is ſomething in the tooth-like pieces, which may be pleaded in behalf of all the reſt of thoſe which were ſent us; ſome have at leaſt equal claim to be in teeth, with any *gloſſopetræ*; they have all the *perioſteum* or enamel thereof, which was kept perfectly, though the bony part covered by this enamel, and the root of the tooth which never had been covered thereby is a meer white ſtone, which by the application of fire turns blue and becomes a turquois*.

The figure of theſe teeth however is not like that of the *gloſſopetræ*; the former are flattiſh like the *molares* of an animal, whereas the latter are acute like *inſiſores*. They are ſome of them of a prodigious ſize, I have ſeen one almoſt as big as the fiſt, but the ſmall ones are much more numerous; they often happen to have a very little of the turquois matter in them, but are much what marcaſites, are in other mines, they are called acordingly by that name, and are even looked upon as indications of metals,there are two ſpecies among the little teeth ſome with four principal eminences, diſpoſed at the four corners of a ſquare†; and in theſe, when the ſide oppoſite to the eminences, which is that whereby the tooth adhered to the jaw is not filled up with matter, we find 4 cavities, which open into the 4 eminences, and probably had received the nerves of the tooth‖.

The other ſpecies of ſmall teeth have alſo four cavities on the ſide next the jaw, but they have

* Plate II. Fig. 1. † Fig. 3 and 5. ‖ Fig. 4.

only two eminences which are both of them triangular, and the origin of each is a ſemi-cylindrical cavity*.

The exact figure of the large teeth is very difficult to learn, by reaſon we very rarely meet with them entire. M. *de Juſſieu* has given us a draught of one of them, formerly in the cabinet of M. *de Monconys*, which is ſomewhat different from thoſe we have ſeen†, and may give us room to imagine that there are different ſpecies among the large teeth, as well as among the ſmall ones. I have a tooth dug from a mine by M. *de Giſcaro* a gentleman of *Simore*, which is in figure of a cone ſomewhat bent like thoſe uſed by gilders; it has but one aperture for the inſertion of the nerve. No doubt in fine can remain, but that the bony parts of certain teeth petrified form our turquois mines, tho' it may be queſtioned what the animals are to which theſe teeth belong. This at preſent I am not prepared to determine, perhaps it may be found hereafter, as the fiſhes have at length been diſcovered from whence the *gloſſo petræ* or ſuppoſed ſerpents tongues aroſe. It is likely our teeth belonged alſo to ſome ſea animals, there being no terreſtrial ones known with any thing like them. It is probably from the bones of the ſame animal, that the other turquoiſes ariſe, whoſe figure is different from that of teeth. It is affirmed that pieces have been found of this kind weighing 100 lb. but theſe were extraordinary. Two of the laſt that were found weigh'd each about 15 pound, it is difficult to get them out of the earth entire, as being very brittle and ſoftiſh; add that they are ſoaked with moiſture as ſtones commonly are in the quarries, but may be diſtinguiſhed in their bed by their oblong figure, and the roundneſs of their cir-

* Fig. 6 and 7. † Fig. 17 and 18.

cumference

cumference; their thickneſs is uſually equal to that of the arm, and their length to that of the leg or thigh. From this long and roundiſh figure probably aroſe their name of *mineral unicorn*; though *Borel* calls them *petrified* bones.

If their external figure were not enough to prove them to have really been bones, an examination of their ſubſtance would put the matter out of queſtion. It appears at firſt ſight different from other ſtones, and ſeems rather to reſemble ivory or other bony matter; its poliſh is between that of a pebble and a bone, yet notwithſtanding ſuch poliſh it ſticks to the tongue like a bone. Upon a nearer inſpection it appears compoſed of ſcales or layers, which however is no diſtinguiſhing character but a ſtructure common to bones, and to the whole claſs of foliated ſtones. But this is peculiar to it, that theſe *laminæ* only ſerve as it were to form the mould wherein the proper matter of the ſtone is caſt, and the more ſenſible theſe *laminæ* are, the leſs perfect is the turquois, that is, the leſs mature*. The workmen frequently find whole veins which are uſeleſs on this account, and which upon laying pieces of them on the fire divide into thin ſcales, as not having received enough of the ſtony matter, to bind them firmly together, but there are other differences between the diſpoſitions of the layers of ſome pieces of turquois, and thoſe of other ſtones; for upon breaking ſome of thoſe wherein the layers are moſt ſenſible, the edge where thoſe layers terminate appears formed of a multitude of roundiſh flutings, by reaſon the extremes of each layer are round, whereas thoſe of the true foliated ſtones, as ſlate, talc, &c. are always ſharp. Each layer of the turquois ſeems as if compoſed of tubes, placed cloſe to each other,

* Fig. 8 and 9.

and

and that to break them is to ſeparate two of theſe tubes*.

A further difference ſometimes obſervable in the *ſtrata* is that their *contours* are waved or curled, whereas thoſe of other ſtones are rectilinear, or at leaſt maintain a continued curvity, which muſt needs be the caſe in all ſtones formed by the meer appoſition of parts. I have likewiſe obſerved ſome pieces of turquois, where the edges of each layer ſeemed formed of a multitude of different parts, ſeparated from each other by regular intervals, which agrees very well with the arrangement of the cells of bones. I have even known ſome where the horizontal layers were all regularly interſected, by vertical ones, the edges of each being compos'd of ſeparate parts about the bigneſs of large points. In fine, there are ſome veins of matter worthleſs in themſelves, but whoſe very defect may ſerve to diſcover their firſt origin; upon laying them on the fire they become full of little holes or apertures which bear a near reſemblance to the little holes and cells found in bones calcined, or expoſed long to the air. Thoſe here mentioned are ſuch as have not been filled with a matter capable of reſiſting the fire.

Roſnel charges all our turquoiſes with being full of ſpecks, and filaments, and by this he diſtinguiſhes them from thoſe of *Perſia*, whereas in reality this only diſtinguiſhes ſuch of them as are not arrived at the maturity of the reſt, the ſtreaks and filaments being only viſible in thoſe, the intervals of whoſe leaves are not ſufficiently filled with the ſtony matter. Theſe filaments, when viewed by the microſcope, ſhew the thickneſs of the layers, and always affect the ſame direction.

* Fig, 10.

Stones like thoſe now deſcribed being found near the ſurface of the earth, have uſually paſt for indications which determined them to dig farther in hopes of arriving at veins of a like matter, only more mature. Thoſe hitherto diſcovered are on little eminences in waſte ſandy grounds, but few of them were come at without firſt digging a deal. They were uſually obliged to remove a layer of common earth, 2 or 2 feet ½ thick, under which they met with alternate beds of ſand and rock, and they rarely reach the mine, till they had dug upwards of 50 feet deep, tho' the limits of the digging are no more certain here than in other kinds of mines.

The ſand which firſt preſents itſelf, after removing the external ſoil is like river ſand, both as to ſize and colour; after this comes another which ſhews there is a vein behind, it is finer than the former, and of a browner colour, though ſometimes it is found blueiſh; each colour is look'd on as a happy ſign of a mine underneath, whoſe baſe is a whitiſh earth called by the workmen *Beaume*, the pieces are ſurrounded with a cruſt of the fine ſand, and little ſtones intermixed.

To purſue the mine when found they dig vaults under ground, ſuſtained by pillars to prevent the falling in, the greateſt obſtacle they uſually meet with is water, which ſometimes diſables them from purſuing the mine, and ſometimes hinders their arriving at it.

The veins of this, as of other mines, contains ſometimes more and ſometimes leſs matter, ſome of them are four or five inches broad, ſome more and others leſs, and there is the like diverſity in the richneſs of the matter, that is in its diſpoſition to become fine turquois. Not only the matter of different mines, but even of different parts of the ſame mine,

mine, differ much in their colour, ſome of them being of a yellowiſh white, others white bordering on carnation, and others upon brown or greyiſh. This laſt coloured ſtone is moſt valued; but be the colour what it will, it is always very different from that of the future turquois, which is to be given it by fire, but before they expoſe it thereto, it is left in the open air, till it is dry enough to ſtick to the tongue.

To make it take the fine colour, they heat it in a furnace of a peculiar make *. It is much longer than broad, being uſually 8 feet one way, and 1 foot 3 inches the other; its height or depth 1 foot and 4 or 5 inches. At one end is an aperture of the whole height and breadth of the furnace†; and by this the matter is put in. It is heated by a reverberatory fire, the hearth whereon the wood is laid being at the other end, at which place the floor of the furnace is diſcontinued. Its cavity taken from top to bottom is 20 inches more than elſewhere, which holds for about two feet of the length of the furnace; for the reſt, the breadth and covering are the ſame. At bottom is a ſquare aperture||, about 10 inches every way, thro' which the fuel is put. The flame riſing to the roof, or vault, is driven back upon the place where the mineral is laid, and to prevent the flame from touching it ere it have roſe from the bottom of the furnace, there is a ledge ſome inches high at the end of a floor**. The ſame furnace has another aperture in the nature of a window, about 8 inches ſquare††. This is uſually ſtopped with a brick, being only to be opened on particular occaſions. It is an eſpecial caution, that the mi-

* Fig. 19 and 20.
† Fig. 19. A.
|| Fig. 19. D.
** Fig. 20. H.
†† Fig. 19. E.

neral

neral be only heated by degrees. If this be done too haſtily, ſuch of it as is diſpoſed to part, will fall into ſcales; and even the reſt will break into little pieces. The moiſture which ſeparates the layers, requires to be evaporated ſlowly, nor will all the matter ſuſtain an equal quantity of heat, ſome of it taking the blue much eaſier than other. To give the ſuitable degree of heat to all, they place them in a kind of earthen ſlippers* about 8 inches long, and of ſuch breadth as that 2 of them may be placed aſide each other in the furnace. Theſe ſlippers may be conſidered as a ſort of mufflers, like thoſe wherein refiners place their cupels and crucibles, though with an aperture much ſmaller. Two of theſe ſlippers being placed at the entrance of the furnace, are left there half an hour; after which they are advanced a length further, and two others put in the room of the former; and thus they continue advancing them every half hour towards the place of greateſt heat, and place new ones in their room.

It has been obſerved, that all the matter does not take the colour equally quick; and hence they are obliged to take ſpecial notice of the ſeveral changes which happen in each ſlipper. In order hereto they take pieces out with a little ſhovel†, and bringing them to the mouth of the furnace judge hereby of the condition of the reſt in the ſame ſlipper, and accordingly either advance it further, or leave it in its place, as they judge proper. Some of the matter will take its colour in two hours, or even leſs; others require four or five hours; the moſt obſtinate is ſometimes put in at the ſquare aperture, or window, that it may be nearer the main heat. Though a

* Fig. 21. † Fig. 23.

ſurnace be neceſſary to the operators, who have a great deal of the mineral, and this mineral of different qualities to be coloured at the ſame time, thoſe who have only experiments to make in little, or would try, for inſtance, whether a piece of mineral be of turquois or not, may make ſhift without a furnace. The hearth of a common chimney may ſuffice, and a pipe's head has ſometimes ſerved me for a crucible. I had nothing to do, but put the pieces I would colour therein, remove the cinders from the hearth, ſet my little crucible down, ſurround it on all ſides with lighted coals, tho' not ſo cloſe as to touch it. I uſually took the pipe out for the firſt time when it begun to turn red, and examined whether any change was befallen in the colour of the matter.

This I found by experiment was neceſſary to be well attended to, for the fire which gives the blue colour, takes it away again if too long expoſed thereto; the blueneſs of the ſtones increaſes, and takes ſhades ſtill higher and higher, till arrived at a certain point, after which it begins to diminiſh again, and if continued in the fire, grows continually paler and paler, till at length it diſappears, and either takes an ugly greeniſh caſt, or a yellowiſh or blackiſh one, nothing like that of the turquois. It would be eaſy to know when 'tis time to take a ſtone from the fire, if all of them were ſuſceptible of the ſame degree of blue, ſince nothing would be here required but to compare them with a ſtone of a fine colour; but the *maximum*, to uſe a geometrical term, of the blue of one, is not the *maximum* of the blue of another; and the beſt we can do is to take frequent ſamples out of the fire, when they come to a tolerable colour there is no great harm in letting ſuch ſtones looſe their colour, as only take a very weak one.

The

The blue of the *Persian* turquoises is no more proof against fire, than that of ours. I have gathered several broken pieces of these oriental turquoises among the lapidaries, and putting them in pipe-heads, and exposing them to the fire, found their colour usually disappear in less than a quarter of an hour.

A stone does not take colour equally throughout all its parts, not being disposed to attain their respective colours with equal quickness, which is one reason why large turquoises are so very rare, when so many of them are taken out of the mine. These must be kept on the fire longer than others, that they may colour to the centre, which occasions the spoiling of great numbers; add, that the heat of the fire frequently cleaves them into several pieces, which is another reason of their rareness. It would also endanger the splitting even of those pieces which had best bore the fire, if they were exposed too suddenly to the naked air; they require to be cooled with much the same precautions as they did to be heated, tho' at *Simore* they contented themselves with casting hot ashes upon the slippers, and covering the turquoises therewith ere they took the slipper out of the fire, and letting them cool under such ashes.

The pieces of mineral have sometimes another defect which does not appear on their outside, being divided into several parts by thin interstices, lined with a blackish matter, which usually forms a kind of figures in low *relievo*, a defect which naturalists may perhaps take for a curiosity. The figures represented by this black matter came nearest those little stars which give the name to a kind of a stellate stone*; tho' the turquois figures

* Fig. 13, and 14.

are somewhat less regular and thicker withal. I have some pieces wherein this black substance exhibits little plants scarce a line long, yet withal their ramifications very regularly drawn*. Where the stars are still smaller, and very near each other, in some other stones the black matter is much thinner, so as not to assume any regular figure, and yet spoils the turquois nevertheless.

'Tis very natural to enquire why fire should give the turquois matter a blue colour, which accordingly we shall now proceed to consider, and we do it the more willingly as we find no occasion for recurring to very obscure causes, nothing to require from the insensible particles so frequently called upon for solving most physical *phænomena*.———In describing the matter of the turquois, such as it comes from the mine, we omitted to mention a multitude of points, lines, and veins, wherewith it appears interspersed when broken†; these specks, veins, *&c.* are of a colour which borders on black, but what serves to unravel the difficulty, this black has a bluish cast, and appears much like a deep blue. When laid extremely thick the blueness is particularly visible in those places where the layers are very thin; and upon pursuing the small threads with a microscope, we find them all bluish at last.

Now these points and veins may all be considered as so many cells filled with a proper matter for colouring the turquois, they are in effect the cells of bones, which in lieu of being possessed by the stony juice, have been filled with a bluish matter.———All therefore that remains to give the stone an equal tincture throughout, is to pene-

* Fig. 15 and 16. † Fig. 11 and 12.

trate

trate it with ſome fluid, which without deſtroying its texture, may diſſolve the bluiſh matter contained in thoſe cells, and diffuſe it thro' the whole ſubſtance of the ſtone.

And ſuch diſſolvant is the fire, let it not be wondered that I uſe the fire as a diſſolvant of colours, when none but aqueous or oily liquids are commonly uſed for that purpoſe, ſince the different colours wherewith flame is tinged, are ſufficient proof that it diſſolves them. Why may not fire be ſaid to diſſolve verdigreaſe as well as water, when we find the flame of wood, or other matter painted with verdigreaſe green, as well as the water wherein ſuch matter is ſteeped, to procure a flame infallibly green? We need only tinge a piece of paper with verdigreaſe, either fluid or in powder, and ſet fire to it, and M. *Mariotte* obſerves, that upon throwing a bundle of the parings of the edges of hats into the fire, there firſt ariſes a white flame, and then a ſeries of blue, green, and violet colours; the firſt flame is the colour of the ſtuff, the other colours ariſe from the mixture of verditer with other drugs uſed in the dying of hats.

After the like manner we may ſuppoſe, that the fire which penetrates the ſtone ſo as to turn it red, diſſolves or even looſens the matter lodged in the cells, and carries it along in the ſeveral tracts through which itſelf paſſes, leaving ſome of it in every part of the way, by which means what was before collected in little maſſes, is now diſtributed through the whole ſubſtance of the ſtone.

Nor need it be apprehended, that the blue matter lodged in the little cells ſhould not ſuffice to dye the whole ſtone. Colours admit of a prodigious extenſion, and it is ſurpriſing after what infinite diviſion they are ſtill ſenſible. Mr. *Boyle* gives

gives us a *calculus* hereof in his treatiſe of the ſubtilty of effluvia. A grain of copper, he finds capable of giving a blue colour to 28,534 grains of water; or which amounts to the ſame, a grain of copper will colour a bulk of water 256,806 times greater than itſelf.

It is even probable, that if the quantity of blue matter in the mineral were greater, or lodged in greater cells, it will give leſs colour to the turquois. We have obſerved that a paper covered with verdigreaſe yields a green flame, but if the layer of verdigreaſe be too thick, or if a piece of verdigreaſe the bigneſs of a pea be incloſed in the paper the flame will take nothing of its colour, nor would the flame be tinged, when I caſt a large piece upon lighted wood, though it always was when I threw it powdered thereon. The fire which is able to diſſolve and carry off the powder, cannot compaſs a large maſs, as a torch may melt ſilver-wire, tho' too weak to diſſolve a large bar of the ſame metal.

What has been ſaid of the blue matter may be confirmed from ſome further obſervations——I took ſeveral pieces of the crude turquois, ſome whereof had ſtore of ſpecks and veins filled with the bluiſh matter, and others ſcarce any apparent ones; theſe being expoſed to the fire, I always found that ſuch as had the moſt coloured points, took the fineſt colour, as having an ampler fund of the dye; ſome pieces which had ſcarce any viſible points, and probably had but few inviſible ones, ſcarce took any blueneſs at all, which is perfectly agreeable with what was wrote us from *Simore*, as to the different quality of the ſtones; thoſe noted for the beſt were of a grey colour, where the white prevailed leſs than in the reſt. A multitude of points of a deep blue placed near each other

ther, and ſeparated by white afford a blue grey, olour. Accordingly our iron grey ſtuffs, which eally are a blue grey, are made of a mixture of blue nd white wool.

I have not only obſerved, that among ſeveral ieces, thoſe which had moſt veins or points of a leep blue attained to the fineſt colour, but that the laces near the blue veins or points took more coour than thoſe further off; the obſervation was aſy to make, noting ſome of the more remarkable veins or points, before the ſtone was put in the ire. Yet ſome pieces of ſtone I have met with, which tho' they had but few viſible ſpots, came to a tolerable colour; but all we can hence infer, is hat the dye happened here to be diſtributed in very ſmall particles.

The colour of the ſpecks and veins commonly remains deeper than that of other parts of the ſtone, and hence it frequently happens that the colour of our turquoiſes is not equal throughout; there are ſeveral of them ſet wherein the veins and points are ſtill diſtinguiſhable, by the difference of ſhades; whence it follows that the beſt mine is that where the colouring matter is diſtinguiſhed in the ſmalleſt parcels, and neareſt each other; theſe veins however are not held any defect in the turquoiſes, provided their poliſh be not altered thereby, and they are even prized in turquoiſes of the old rock; but it ſometimes happens that ſtones whoſe points or veins are too large, take an ill poliſh, by reaſon their ſurface is filled with ſeveral inequalities and a multitude of little cavities, for the cells which had been filled by the blue colour are left empty, when the ſtone is taken from the fire, and hence their cavities are the more conſpicuous as they had contained more matter.

Boccone

Boccone attributes the change of colour in th turquois to a kind of vitrification, not confiderin that a degree of heat much too feeble to vitrify th matter would give it a blue caſt. To ſupport hi account he mentions ſome lime ſtones in *Sicily* which turn blue in the burning; this fact, thoug no proof of what he aſſerts, is yet remarkable, a it ſhews that ſeveral common ſtones may be ſtain'c blue, like the mineral turquois.

The matter which tinges our turquoiſes is probably found in great abundance in the country about *Simore*; we have cryſtallizations from thence of a beautiful blue colour, which, were they more tranſparent, might be ranked among ſaphires; but ſeem now rather of the nature of the ſtone called by *Boccone* the ſtony and blue concretion of *Tirol.* Its figure he compares to that of pieces of tartar, which alſo ſuits with our cryſtallizations, and he adds that ſome merchants ſell them for turquoiſes; the like of which ſome people pretend to do by our cryſtallizations, though an ordinary eye will eaſily diſtinguiſh them.

It muſt be added that the fire not only gives the turquois its colour but increaſes its hardneſs, either by filling up ſeveral ſmall interſtices therein by the dye, or by exhaling ſome ſuperfluous moiſture, which kept the parts aſunder, or by adding ſome of its own ſubſtance thereto as we find it do in ſome other caſes. It is at leaſt certain that the crude ſtone is much tenderer than that brought to the colour, rubbing one of them againſt the other, the latter will make ſcratches in the former, but receive none from it.

A paſſage of *Guy de la Broſſe* quoted at the beginning of this memoir, led us to make ſome experiments upon the mineral unicorn, ſuch as commonly is found in the ſhops. That which we us'd

was

was much ſofter than the mineral turquois, and withal was whiter, having ſcarce any blue ſpots or veins, accordingly the fire ſcarce brought it to any ſenſible blueneſs, though it encreaſed its hardneſs.

The coloured matter which fills the cells of the turquois, and afterwards tinges the whole ſtone, is doubtleſs a mineral, but whether it be a meer mineral, as *Colbet*, or the matter whereof an azure or zafre is made, which gives the fine blue to *China* and *Delpht* ware, or whether it be a metallic ſubſtance, is a point I am not yet ſatisfied about, tho the matter which tinges our turquoiſes, ſeems to me different from that which tinges the *Perſian* ones.

If our turquois mines were wrought again, and obſervation made of the nature of the ſoil about them, it is probable mines would be diſcovered of the mineral which furniſhes this beautiful blue, and no doubt but it would pay richly for the expence of the purſuit. The mines which yield the azure and zafre are very profitable to *Germany*, and thoſe near *St. Mary* in *Alſace* afford a conſiderable income to *France*.

At firſt I ſuſpected that our turquoiſes might derive their colour from copper, a metal very fit to give blue and green, which makes diſſolutions of ſilver bluiſh, and probable gives the colour to emeralds. Some credible authors affirm that upon rubbing them on a touchſtone they leave yellow marks, like thoſe of a copper matter, but the experiment never ſuceeded with me either in turquoiſes or emeralds.

But the dye of a turquois, I found might be diſcharged like that of coral; of all the diſſolvants I tried with this view diſtilled vinegar anſwered the beſt; if a piece of turquois be ſteeped herein for two or three hours, its corners will be turned white

and in two or three days all the outside of the stone, and even almost all its inside will come to the same colour. At the same time that the vinegar takes away the colour, it causes some solution of the stone, which accordingly we find coloured with a kind of white cream, composed of particles separated therefrom. The juice of citron does likewise dissolve this stone, but it only weakens their colour, and what is found under the kind of cream abovementioned, is blue when the stone is laid in this juice.

As to aqua fortis and aqua regia, they dissolve the substance of the stone very readily, but are not so fit to extract its dye; they afford us however a method of distinguishing the *Persian* turquoises from those of *French* growth; for *aqua fortis* does not act at all upon the former, and hence it follows that the two stones tho' similar in appearance, are of a different nature, but no consequence hence is to be drawn to the disadvantage of ours, as if they were softer than the other, for *aqua fortis* which preys upon iron has no effect on wax.——*Aqua regia* also acts differently on the two stones, for it dissolves ours entirely, and reduces those of *Persia* into a kind of paste, much whiter than the turquois was, tho' not altogether destitute of its blue colour. One might almost suspect that there is gold in the dye of the *Persian* turquois, at least there must be some matter which *aqua regia* acts on, but acts after the same manner as upon a mass or mixture of gold and silver.

But the great failing of all these stones is, that they will change their colour, without any other dissolvant than time and the air; thus we find their blue colour gradually take shades which border on green untill they become greenish, and at length down right green; whereas the colours of other

precious ſtones is immutable. When the turquoiſes are turned green they are of no further value; the common opinion having not thought fit to ſet any value on them with this green colour. If the blue of ours were more durable than that of the *Perſian* ones, as *Berquen* affirms, it would be a great advantage, but this it is not eaſy to be ſatisfied of by experiment, which would require a large ſeries of years; and yet thoſe of *Perſia* ſeem more diſpos'd to turn green, while the blue of ours turns white in diſtilled vinegar, thoſe of *Perſia* become green.

Diverſe means have been attempted tho' with little ſucceſs for reſtoring the faded turquoiſes to the blue colour; the beſt is to grind a thin piece of the ſtone, and poliſh it anew, for the change of colour begins in the ſurface as being moſt expoſed to the air, and frequently the green penetrates no further; in which caſe by leſſening the bulk of the ſtone a little, it recovers its firſt beauty. Moſt of the other means mentioned by certain authors are more apt to change the green of a turquois into a pale blue, than to reſtore it to its former colour. Thus after a well known preſcription, putting a piece of a *Perſian* turquois which was turned in *aqua fortis*, the green diſappeared in 24 hours, but the blue which ſuceeded it was ſo feeble that the turquois was no better blue than when green.

We ſhall not ſet the turquoiſes off by the fictitious virtues aſcribed to them, tho' a great many fine things might be ſaid on this head from very grave authors. It is affirmed that the ſtone draws the misfortunes to itſelf, which would otherwiſe fall on its maſter. Of this *de Boot* gives an inſtance which he thinks very deciſive, his horſe falling with him from a high place into a hollow way, he found his turquois broken, a mighty wonder for a ſoft ſtone?

but as to himſelf he caught no harm. *Wormius* declares that he received the ſame benefit from a turquois, and that his adventure was ſo like that of *de Boot*, that he dare not relate it for fear of ſuſpicion of having ſtole it. Some perſons doubtleſs will laugh to hear that it does not agree with married people, but breaks on their fingers. That it ſhows all the changes which befal in the body of the perſon that bears it by ſuitable change of colour, and that this has kept it from being us'd among other ſtones in the ladies attire, as only ſuiting them to a certain age.———The relating ſuch fables is enough to refute them, and ſome may even reproach us for ſuch a refutation.

An explanation of the figures in Plate II. *tranſlated by* J. M.

Fig. 1. is a large tooth lately taken from the mines about *Simore*, a a a b b is the part covered with the enamel.

c c c mark the place where the enamel ends, and the ſtony and mineral matter begins.

d d d, c c c is the part which is mineral.

e e is the place where the tooth was broken.

f marks that which is occupied by the mineral.

Fig. 2. is the ſame tooth ſeen on the oppoſite ſide.

g g g ſhew that which is covered with enamel.

h h, i i i where it was broken, which is mineral.

At k there are theſe figures which imitate vegetations.

Fig.

Fig. 3. is a little tooth of the firſt ſort, which on the ſide ſhewn in the figure, diſcovers nothing but enamel.

q q q q are the 4 eminences of this tooth.

Fig. 4. is the ſame little tooth reverſed, and ſeen on the ſide which adhered to the jaw; it has but little mineral matter.

r r r r mark the 4 holes wherein the nerves were inſerted.

Fig. 5. is another little tooth of the firſt ſort, but having the 4 eminences s s s s more pointed.

Fig. 6. is a tooth of the ſecond ſort, viewed two different ways.

t t are its two eminences.

u u the cavities which are at its origin.

Fig. 7. is a conical tooth, x is the hole of the inſertion of the nerve.

Fig. 8. is a piece of mineral turquois, wherein the *ſtrata* or *laminæ*, of which it is compoſed, appear on the upper ſurface; their direction is in a right line.

Fig. 9. is a piece where the *ſtrata* are undulated.

Fig. 10. is a piece where the horizontal *ſtrata* are croſſed by vertical ones, and where theſe *ſtrata* form rounded flutings.

Fig. 11. is a piece where the lines and points ſhew the diſpoſition of the black or dark blue veins and points, which we have called the reſervoirs of the matter which tinges the turquoiſes.

Fig. 12. is a ſmall part of the ſame piece magnified.

Fig. 13. is 2 pieces n and y detached from each other, between which a blackiſh matter formed ſome ſorts of little ſtars.

Fig.

Fig. 14. is the piece y magnified, to render the ſtars more ſenſible.

Fig. 15. is a piece of mineral, wherein the black matter repreſents a little plant.

Fig. 16. is this plant ſeen ſeparately.

Fig. 17. is the tooth which M. *de Juſſieu* cauſed to be drawn at *Lyons*.

l l is the tooth.

m m, n n is a piece of the turquois matter, which here perhaps is a part of the jaw.

Fig. 18. is the ſame tooth ſeen on the other ſide.

p p is the bony part of it.

The ſcale ſhews the ſizes of *fig.* 17 and 18.

Fig. 19. is the furnace for colouring the turquois, in perſpective.

A is the aperture at which the ſtone is put in.

B B ſhews where the table or bottom of the furnace begins.

C C is the vault of it.

D the aperture at which the wood is put in.

E a ſort of window by which they look into the furnace; it ſerves alſo to put in the moſt ſtubborn mineral.

Fig 20. is a ſection of the ſame furnace.

F. its aperture.

G G the table.

H H the place where it ends.

I I a little rim which obliges the flame to riſe.

K the place where the wood is put in.

Fig. 21. is the ſlipper in which the mineral is put.

Fig. 22. is the fork which ſerves to puſh the ſlippers into the oven, and to take them out again.

Fig.

Fig. 23. is a little ſhovel, with which they take little pieces out of the ſlippers, to examine whether they have taken colour.

IV. *Reflections on ſeveral obſervations of the eclipſe of* Jupiter, *and his ſatellites, by the moon, made at* Rome, Marſeilles, *and* Nuremberg, *by M.* Caſſini*.

We are already ſufficiently informed of the uſefulneſs of the eclipſes of the planets and fixed ſtars by the moon for the diſcovery of the longitudes. We have ſeveral times related them to the academy, and they have ſerved us to determine the difference of the meridians between the different places where they have been obſerved.

The laſt obſervation of the eclipſe of *Jupiter* by the moon gives us a favourable opportunity for this determination; it was obſerved at *Rome* by M. *Bianchini*, at *Marſeilles* by F. *Laval*, and F. *Feuillée*, and at *Nuremberg*, by M. *Wurzelbaur*. In theſe two laſt cities they have obſerved, as well as at *Paris*, the emerſion of the ſatellites of *Jupiter* from the dark limb of the moon, which was perceived with a very great exactneſs, and almoſt at the ſame inſtant by ſeveral obſervers in the ſame place. We have alſo thought it proper to give the method of calculating the obſervations of theſe ſatellites, and of uſing them to determine the difference of the meridians, which will ſerve for a ſupplement to the general method which has been given in the memoirs of the academy for 1702, to determine the longitudes by means of the eclipſes of the fixed ſtars and planets by the moon.

* *Dec.* 20, 1715.

To

To determine the hour of the immerſion of each ſatellite, we muſt firſt determine the ſituation with regard to the centre of *Jupiter*, for ſome hours before and after their conjunction with the moon, which may be done by the tables of the ſatellites of *Jupiter*, or by means of the figures which have been prepared on purpoſe to ſhew their configuration. Reduce their diſtance from the centre of *Jupiter* into minutes of a degree, which being added to the place of *Jupiter*, when the ſatellite is to the E. or ſubſtracted when it is to the W. will give the true longitude of this ſatellite.

With regard to its latitude, we may without any ſenſible error, ſuppoſe it the ſame with that of *Jupiter*.

The longitude and latitude of the ſatellites being known, we ſhall have their right aſcenſion and declination, and having determined the time of their true conjunction, we may calculate their eclipſes after the manner explained in the memoirs of 1702, having regard to the motion of each ſatellite from the time of its true conjunction to the time of its apparent immerſion or emerſion.

Now to determine the difference of the meridians between the different places where the obſerſervation has been made, we ſhall make uſe of the parallel of each place drawn in the figure prepared for the calculation of the eclipſe of *Jupiter*; for though the declination of theſe ſatellites is not exactly the ſame with that of the centre of *Jupiter* the difference that may be therein being but a few minutes, is too ſmall to be regarded. We ſhall make uſe alſo of the orbit of the moon drawn in the projection.

Take with a pair of compaſſes the ſemi-diameter of the moon, and having placed one of the points upon a parallel, as that of *Paris*, the hour

points


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
Fig. 16.
Fig. 17
5 Inches.
6. Lines
6. Inches
2 Inches.
Fig. 18.
3 Inches.
Fig. 19.
Fig. 20.
Fig. 22.
Fig. 23.
Fig. 21.
1 2 3 4 5 6 Inches.

J. Mynde sc.

that the ſatellite appeared to enter into the moon, and deſcribe at the interval of the ſemi-diameter of the moon an arch of a circle toward the W. which will cut the orbit of the moon at a point which will mark the centre of the moon, at the moment of the immerſion; having alſo placed the point of the compaſſes on the ſame parallel at the hour that the ſatellite appeared to emerge, deſcribe at the ſame interval an arch of a circle towards the E. which will mark the centre of the moon at the moment of the emerſion. Divide this orbit into hours and minutes equal to thoſe made uſe of for the calculation of the eclipſe of *Jupiter*, marking at the points of the occidental interſection the hour of the immerſion of the ſatellite, and at the point of its oriental interſection the hour of its emerſion.

Afterwards upon the place where the correſponding obſervation was made, ſet one point of the compaſſes at the hour when the immerſion or emerſion was obſerved, and deſcribe at the interval of the ſemi-diameter of the moon an arch of a circle which will cut its orbit. The difference between the hour marked by this interſection, and the hour of the obſervation is the difference bebetween the meridian of *Paris*, and that of the place where the obſervation was made.

Ex. *At* Marſeilles, July 25, 1715, *by F.* Laval.

h	′	″	
2	19	38	emerſion of the third ſatellite of *Jupiter* from the dark limb of the moon.
2	24	9	emerſion of the ſecond ſatellite.
2	25	53	emerſion of the firſt, which is the neareſt.
2	25	56	emerſion of *Jupiter* from the dark limb of the moon.

 Having

	h	′	″
Having firſt divided the orbit of the moon, ſo that the hour of the emerſion of the third ſatellite at *Paris* which happened at	2	10	22
Was diſtant from the ſame hour marked on the parallel of *Paris*, the interval of the ſemi-diameter of the moon, we placed a point of the compaſſes upon the parallel of *Marſeilles*, at the hour of the emerſion of the third ſatellite, which happened at	2	19	38
And we deſcribed with theſe compaſſes, at the interval of the ſemi-diameter of the moon, an arch of a circle which cuts its orbit at —	2	6	50
The difference between theſe hours, which is ——	0	12	48

Repreſents the difference of the meridians by which *Marſeilles* is more to the eaſt than *Paris*, becauſe the hour of the obſervation at *Marſeilles*, is greater than that which is marked upon the orbit.

	′	″
Having divided in like manner the orbit of the moon for the obſervations of the other ſatellites of *Jupiter*, we found by the emerſion of the ſecond, the difference of the meridians to be ——	12	20
By the emerſion of the firſt.	12	24
By the emerſion of *Jupiter*.	12	9
Taking a mean between theſe different determinations, we ſhall have the difference of the meridians between *Paris* and *Marſeilles*. ——	12	25
Or — —— 3°	6	15

Which is not far from 45″ of a degree, as it is marked in the *Connoiſſance des Temps*.

At Nuremberg, July 25, 1715.

h	′	″	
2	8	38	the total immerſion of *Jupiter* into the moon.
2	52	55	emerſion of the third ſatellite.
2	58	20	emerſion of the firſt.
3	0	0	emerſion of *Jupiter.*

Having in like manner divided the orbit of the moon according to theſe different obſervations, we found by the immerſion of *Jupiter* the difference of the meridians between *Paris* and *Nuremberg* to be — — 35 23

We determined alſo by the emerſion of the third ſatellite the difference of the meridians between *Paris* and *Nuremberg* to be — — 35 40

By the emerſion of the firſt 35 45

And by that of *Jupiter* — 35 0

Taking a mean between theſe different determinations, we ſhall have the difference of the meridians between *Paris* and *Nuremberg* — — 35 27

By which *Nuremberg* is more to the eaſt than *Paris.*

This difference is greater by 31″ of an hour than that which is ſet down in the *Connoiſſance des Temps.*

At Rome, July 25, 1715.

h	′	″	
1	57	16	beginning of the immerſion of *Jupiter* into the moon.
1	58	26	total immerſion of *Jupiter.*
2	56	26	beginning of the emerſion of *Jupiter.*

By the obfervation of the total immerfion we fhall find the difference of the meridians between *Paris* and *Rome*	′ 40	″ 11
And by the beginning of the emerfion	40	31
Of which the mean is	40	21

Lefs by 59 feconds of an hour than that which is marked in the *Connoiffance des Temps.*

V. *A comparifon of the obfervations of the eclipfe of the fun,* May 3, 1715, *made in feveral cities of* Europe, *by M.* Caffini*.

The perfection of the theory of the fun and moon is not the only advantage that can be drawn from the obfervations of the eclipfes of the fun. They ferve alfo very ufefully to determine the longitudes of the feveral cities where they have been obferved, by comparing them with the figure prepared for *Paris*, after the manner that has been explained on different occafions.

As the laft eclipfe of the fun was to be total in feveral places, and very confiderable for its magnitude through almoft all *Europe*, it has excited the curiofity of feveral learned men, whofe obfervations have been communicated to us. We fhall here relate the abftract of them, with the hour of the phafes according to the figure prepared for *Paris*, to know the difference of the meridians.

* Dec. 20, 1715.

At

At London *by the* Chevalier de Louville.

	At *London.*			At *Paris* by the figure.			Diff. of the meridian between *Par.* and *Lond.*		
	h	′	″	h	′	″	h	′	″
Beginning at	8	6	13	8	15	30	0	9	17
Total immerſion at	9	9	13	9	18	58	0	9	37
Beginning of the emerſion at	9	12	35	9	21	50	0	9	15
End of the eclipſe at	10	20	19	10	29	40	0	9	21

Taking a mean between theſe differences, which agree together within ſome ſeconds, we ſhall have the difference of the meridians between *Paris* and *London* to be 9′ 23″ of an hour, or 2° 20′ 45′, by which *London* is more to the weſt than *Paris*, becauſe the hour marked by the figure is greater. We have in our *Engliſh* journey determined the difference of the meridians between the obſervatory at *Paris* and that in *England*, to be 9′ 10″, and between *Paris* and the weſtern extremity of *London* to be 9′ 40″. The difference of the meridians which we have juſt found to be 9′ 23″ by the eclipſe of the ſun is the mean between theſe two obſervations, agreeable to the ſituation of the place of obſervation, which was made at *Greſham* college in the eaſtern part of *London*.

At

At Montreuil, *by M.* de Vilmareſt.

	At *Montreuil.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Mont.*		
	h	′	″	h	′	″	h	′	″
1 digit at	8	15	40	8	19	30	0	3	50
6 digits at	8	43	40	8	46	20	0	2	40

The magnitude of the eclipſe was obſerved to be 11 ½ digits.

	At *Montreuil.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Mont.*		
	h	′	″	h	′	″	h	′	″
5 digits at	9	58	40	10	1	35	0	2	55
3 digits at	10	10	40	10	13	5	0	2	25

Taking a mean between theſe differences, we ſhall have the difference of the meridians between *Paris* and *Montreuil* 2′ 57″ of an hour, or 0° 44′ 15″, by which *Montreuil* is more weſterly than *Paris*; which agrees pretty exactly with the beſt maps of *France*.

At Montpellier, *by Meſſ.* de Plantade, *and* Clapiés.

	At *Montpellier.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Mont.*		
	h	′	″	h	′	″	h	′	″
1 digit at	8	17	40	8	10	40	0	7	0
2 digits at	8	22	1	8	15	55	0	6	6
The end exactly at	10	28	37	10	23	30	0	6	23

Taking a mean between theſe differences, we have the difference of the meridians between *Paris* and *Montpellier* 6′ 24″, or 1° 36′, by which *Paris* is more to the weſt.

At Marſeilles *by F.* Laval, *and F.* Feuilleé.

	At *Marſeilles.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Mar.*		
	h	′	″	h	′	″	h	′	″
Beginning at	8	18	36	8	5	40	0	12	56
End at	10	35	24	10	23	35	0	11	49

The magnitude of the eclipſe was obſerved to be 9 digits, 40 minutes.

Taking a mean between theſe two determinations, we have the difference of the meridians between *Paris* and *Marſeilles* 0^h 12′ 23″, or 3° 5′ 45″.

At Recicourt *near* Verdun, *by the* Abbé Teinturier.

	At *Recicourt.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Reci.*		
	h	′	″	h	′	″	h	′	″
Beginning at	8	26	12	8	14	30	0	11	42
End at	10	42	57	10	32	25	0	10	32

The difference of the meridians between *Paris* and *Recicourt*, according to theſe obſervations, is 0^h 11′ 37″, or 2° 54′ 15″, by which *Recicourt* is more to the eaſt.

At Lindau *near the lake of* Conſtance, *by M.* Gaupe.

	At *Lindau.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Lind.*		
	h	′	″	h	′	″	h	′	″
End of the eclipſe at	11	1	15	10	35	15	0	26	0

This

This difference being converted into degrees we ſhall have *Lindau* more eaſterly than *Paris* by 6°. 30′.

At Frankfort *upon the* Main.

	At *Frankfort.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Frank.*		
	h	′	″	h	′	″	h	′	″
Beginning at	8	50	0	8	19	45	0	30	15
End at	11	10	0	10	40	5	0	29	55

The magnitude of the eclipſe was obſerved to be 10 digits, 34 minutes.

Taking a mean between theſe differences, we ſhall have the difference of the meridians between *Paris* and *Frankfort* 0ʰ 30′ 5″, or 7° 31′ 15″, by which *Frankfort* is more eaſterly than *Paris.*

At Hamburgh.

	At *Hamburgh.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Hamb.*		
	h	′	″	h	′	″	h	′	″
Beginning at	8	57	0	8	26	10	0	30	50
The greateſt darkneſs at	10	5	30	9	34	30	0	31	0

The magnitude of the eclipſe was obſerved to be 11° 30′.

According to theſe obſervations, the difference of the meridians between *Paris* and *Hamburgh* is 0ʰ 30′ 55″, or 7° 44′, by which *Hamburgh* is more eaſterly than *Paris.*

At Keil *in* Holstein, *by M.* Reyker.

	At *Keil.*			At *Paris* by the figure.			Diff. of the meridians between *Paris* and *Keil.*		
	h	′	″	h	′	″	h	′	″
Beginning at	9	14	0	8	30	0	0	44	0
End at	10	29	0	10	48	40	0	40	20

The magnitude of the eclipse was observed to be 11 digits, 20 minutes.

The difference of the meridians between *Paris* and *Keil*, which results from the end of the eclipse, does not agree with that of the beginning, which gives room to conjecture, that some mistake has slid into the hour of the observation of *Keil*, which is so much the more manifest, because at *Hamburgh*, which is not one degree more westerly than *Keil*, the beginning appeared 17 minutes sooner, which ought to give almost 4 degrees difference of meridians.

At Berlin, *by M.* Hoffman.

	At *Berlin.*			At *Paris* by the figure.			Diff. of the meridians between *Paris* and *Berlin.*		
	h	′	″	h	′	″	h	′	″
The greatest darkness at	10	22	0	9	36	40	0	45	20
The end at	10	34	1	10	48	25	0	45	36

The magnitude of the eclipse was observed to be exactly 11 digits.

According to these observations, the difference of the meridians between *Paris* and *Berlin*, is 0h 45′ 28″, or 11° 22′, by which *Berlin* is more easterly than *Paris*.

At Dantzick *by M.* Hecker.

	At *Dantzick.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Dantz.*		
	h	′	″	h	′	″	h	′	″
Beginning at	9	40	0	8	34	30	1	5	30
End at	0	2	40	10	56	50	1	5	50

The quantity of the eclipſe could not be obſerved becauſe of the clouds.

According to theſe obſervations, the difference of the meridians between *Paris* and *Dantzick*, is $1^h\ 5'\ 40''$, or $16^\circ\ 25'$, by which *Dantzick* is more eaſterly than *Paris*, which agrees exactly enough with the difference of the meridians between theſe 2 cities, which we have already determined by other obſervations.

At Warſaw.

	At *Warſaw.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Warſ.*		
	h	′	″	h	′	″	h	′	″
Beginning at	9	50	0	8	34	30	1	15	30
5 digits at	11	42	0	10	27	0	1	15	30

According to theſe obſervations, the difference of the meridians between *Paris* and *Warſaw*, is $1^h\ 15'\ 15''$, or $18^\circ\ 48'\ 45''$, by which *Paris* is more weſterly than *Warſaw*.

At

At Upſal *by M.* Vallerius.

	At *Upſal.*			At *Paris* by the figure.			Diff. of the meridians between *Paris* and *Upſal.*		
	h	′	″	h	′	″	h	′	″
Beginning at	9	47	50	8	42	30	1	4	20
Total immerſion at	10	58	15	9	49	35	1	8	40
Beginning of the emerſion	11	2	24	10	53	20	1	9	4
End of the eclipſe at	11	55	40	10	57	20	0	58	35

The difference of the meridians which reſults from theſe different phaſes, does not agree together, which gives room to conjecture, that ſome miſtakes ſlipped into the determination of the time.

The ſun was entirely eclipſed at *Upſal* during the ſpace of 4′ 9″; ſo that the eclipſe was more central there than at *London*, when the duration of the total darkneſs was but 3′ 22″.

At Bologna *by M.* Manfredi.

	At *Bologna.*			At *Paris* by the figure.			Diff. of the meridians between *Par.* and *Bolog.*		
	h	′	″	h	′	″	h	′	″
The beginning doubtful at	8	50	0	8	13	25	0	36	55

 At

At Rome *by M.* Bianchini,

	At *Rome.*			At *Paris* by the figure.			Diff. of the meridians between *Paris* and *Rome.*		
	h	′	″	h	′	″	h	′	″
The eclipse was already begun at	8	53	30						
1 digit at	8	58	13	8	17	0	0	41	13
End of the eclipse at	11	13	13	10	31	50	0	41	23

According to these observations, the difference of the meridians between *Paris* and *Rome*, is 41′ 18″, or 10° 19′ 30″, by which *Rome* is more easterly than *Paris*.

A

A TABLE OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXVI.

In the HISTORY.

In the MEMOIRS.

of like colour; on occasion of which it is attempted to explain the formation of the scales of fishes, by M. de Reaumur.

VII. *A solution of some difficulties relating to the formation and growth of shells, by M.* de Reaumur.

VIII. *A memoir for the construction of a pump which supplies the water continually into the reservoir, by M.* de la Hire, *the younger brother.*

IX. *A description of an addition to be made to windows, to hinder the rain from getting into a room when they are shut, by M.* de la Hire, *the son.*

A N

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1716.

I. *On the origin of ſtones; tranſlated by Mr.* Chambers.

IT is a point fully proved, that all ſtones without exception were originally fluid, or at leaſt ſoft like paſte, and ſince dried and hardened; this any perſon might be convinced of by ſeeing one ſtone wherein ſome foreign body is incloſed, which could not have entered the ſame if it had always retained the ſame conſiſtence; for ſuch one ſtone will conclude for all the reſt, but we have multitudes of them, and every day diſcovers more and more; beſides that there are numbers of thoſe called figured ſtones, all caſt or moulded very exactly in different kinds of ſhells, which ſhews that the paſte they were formed off muſt have been extremely ſoft and pliable.

This well known principle M. *Geoffroy* endeavours to carry further, and gives us the detail of the formation of different kinds of ſtones.

He firſt lays it down that meer earth without any mixture of ſalts or ſulphurs is the only neceſſary baſis of this formation, that this earth may indeed be intermixed with ſalts and ſulphurs, but that it does not need them, and that there are ſome ſtones which

which contain none, as the common ſtones found in quarries, or the ſpecies of white tranſparent flints; there are ſome flints which when ſtruck againſt each other yield a ſenſible ſmell of ſulphur, and conſequently contain a ſulphurous matter, or a vitriolic acid; others are coloured, and therefore contain metalline parts which is the general cauſe of the colours of precious ſtones; but all this is only accidental, as may appear hence, that ſaphires and all our emeraulds of *Auvergne* looſe their colour by a very moderate heat, without any loſs either of their hardneſs or tranſparency, and thus turn meer cryſtals.

The moſt ſimple homogeneous, and therefore moſt perfect of all ſtones according to M. *Geoffroy* is cryſtal of the rock. At firſt ſight one would hardly take it to be earth, which yet it muſt be, for we are certain it is not congealed water as the antients imagined———M. *Geoffroy* ſuppoſes two kinds of primitive particles in earth, ſome extremely thin and ſlender in manner of *lamellæ*, and others of all the ſorts of irregular figures; when the particles of the former kind happen by any means to be found in ſufficient quantity together, the regularity and equality of their figures makes them range themſelves equably and regularly, and thus form a homogeneous compound, which is withal very hard on account of the immediate contact of the parts, and tranſparent by reaſon of their regular diſpoſition, which leaves th elight a free paſſage every way,and this is cryſtal; as to the earthy particles of the ſecond kind it is evident any compoſitions ariſing therefrom muſt be leſs hard, and withal opaque.

Cryſtals then are formed only of particles of the firſt kind, and all other ſtones of particles of both the kinds intermixed; thoſe of the former unite

and bind thoſe of the latter, without which they would be little other than ſand or duſt, and conſequently their conſiſtence and hardneſs ariſes herefrom, but this will need ſome further explication.

Water is a vehicle proper for conveying the earthy particles of the firſt kind, as we find by certain petrifying ſprings which line the canals they run through with ſtone, or even incruſtate any hard bodies left therein for a continuance of time. Water properly ſpeaking does not diſſolve theſe earthy particles, but only keeps them in fuſion, as it does likewiſe by thoſe numerous juices wherewith plants are fed——In purſuance of this analogy, M. *Geoffroy* calls theſe particles of the firſt kind, cryſtalline or ſtony juice.

This juice is heavier and more fixed than water, and does not evaporate with it, which is the origin of the formation of cryſtal; as it is of the ſaline cryſtallizations in chymiſtry. Thoſe cryſtallizations only ariſe, with their ſeveral regular figures, when a water impregnated with ſalts has evaporated ſlowly, and been afterwards expoſed in a moiſt place, the evaporation is neceſſary, that the water by its too great quantity may no longer keep the ſalts too far aſunder; the ſlowneſs of this evaporation is alſo neceſſary, that the ſalts may approach one another gently, and have time to arrange themſelves in ſuch manner as is moſt ſuitable to their figures, without which they would be confuſedly huddled one on another; and a cool place by ſlowly diminiſhing the natural and inteſtine motion of the fluid, contributes at once to both thoſe effects———The application of this to rock cryſtal is eaſy, we have only to conceive a water impregnated copiouſly with the cryſtalline juice, inſinuating through the clefts of a rock and falling

at length to the bottom of a grotto, where it evaporates ſlowly.

We are to conceive the cryſtalline juice, as unequally diffuſed about the earth, hence it is that rock cryſtal does not ariſe in all places, not to mention the other circumſtances which do not often meet together.

If water impregnated with this cryſtalline juice happen to penetrate a piece of common earth, which is the moſt uſual caſe, it will bind the particles thereof by means of this juice, and afterwards as it evaporates, the compound will grow ſtill harder, and at length become ſtone. This will more reſemble cryſtal, that is, be more hard and leſs opaque, as the quantity of cryſtalline juice is greater, and at the ſame time will be of a finer grain as the maſſes of earth were ſmaller and more homogeneous; of this kind are marbles, ſome whereof as well as ſome alablaſters have veins or threads ſo tranſparent that they ſeem little wanting of cryſtal itſelf. The ſtones oppoſite to theſe, and at the ſame time moſt imperfect, are chalk and boles, which are no other than earth half bound together, by a very ſmall quantity of cryſtalline juice and we find them fryable; accordingly the degrees between theſe extremes are eaſy to be conceived.

Some ſtones melt by a large fire as flints, while others only calcine therein, as that whereof plaiſter is made. The principle of fuſion in ſtones is the cryſtalline juice, whoſe particles being equal and homogeneous among themſelves, are diſpoſed to make a continued uninterrupted whole, which if thoſe parts be intimately divided, and violently agitated will be a fluid; in order hereto they muſt be in a ſufficient quantity, and withal capable of being ſeparated from the earthy particles of the ſecond kind, which are too different from them; and

and hence there is no fuſing either of a ſtone, which has but little of the cryſtalling juice, nor of another which though copious enough in juice, is ſo cloſely bound with the other earthy parts, that it always carries them with it. All that can enſue in either of thoſe caſes, is a bare calcination, that is, a confuſed heap of little particles of the ſtone diſjoyned from each other.

The particular circumſtances which accompany the formation of ſtones, varies the effect of the general principles in a multitude of ways.——For an inſtance, if a quantity of the cryſtalline juice mixed with water have been incloſed in earth, and ſuch juice be in too ſmall a quantity to petrify the whole earth; when the water evaporates a maſs will be formed partly cryſtalline and tranſparent, and partly opaque and earthy, and if the cryſtalline juice have remained in the middle of the maſs, the middle alone would be tranſparent, the reſt remaining an opaque cruſt; ſuch are agats and ſeveral other ſtones; on the contrary, if the cryſtalline juice be by any accident driven from the centre to the circumference, there will be pure earth found in the middle of a very tranſparent ſtone; ſuch are ſeveral flints, in whoſe centre we either find a ſoft pulpy earth, or where the water has evaporated ſufficiently, a dry duſt when the petrefaction of any maſs has been diſcontinuous; one part being petrified at one time, and another at a conſiderable diſtance from it, the ſtone will be diſpoſed in coats or *ſtrata* eaſily diſtinguiſhable, as the trunks of trees are for the ſame reaſon, their growth being likewiſe interrupted during the winter.

What has hitherto been ſaid is eaſily applicable to large maſſes of ſtone or quarries, which are found in a multitude of different places.

The younger M. *de la Hire*, related ſome obſervations which give great countenance to M. *Geoffroy*'s ſyſtem——He deſcended into a quarry near the *Fouſſe-porte St. Jacques*, where the whole height of ſtone was perhaps 20 foot, but this height was not all ſtone being interrupted by other *ſtrata* nearly horizontal, as well as thoſe of the ſtone, and of the ſame colour but of a ſubſtance much tenderer and fatty; and which did not harden in the air as ſoft ſtone uſes to do; it is called *Bouſin* and found in all the quarries about *Paris*. Now M. *de la Hire* conceives the floods of water had on ſome occaſion, for inſtance in winter, carried away different matters, which at length were all detained in ſome ſpot, here being at reſt, the heavieſt precipitated to the bottom and formed a bed of ſtone, while the latter remaining a top formed the *Bouſin*; a ſecond flood enſuing in another winter, lays two other ſuch *ſtrata* over the former, and thus on till the whole aſſemblage was formed.

In the ſame quarry M. *de la Hire* obſerved waſte ſtone, covered all over with a tranſparent whitiſh coat, which had bound all the ſtones together great and ſmall, and near this were ſeveral little maſſes incruſtated with the ſame matter, the ſubſtance of ſome whereof was flint, and of others a little piece of ſtone from the uppermoſt bed of ſtone, which is left to ſuſtain the earth, and is ſupported by pillars placed at proper diſtances, fell a large quantity of water through certain natural clefts, ſo as to form a baſon 4 or 5 feet in diameter, and 7 or 8 inches deep, which however could not contain all the water but let it run over at the brim; this brim was incruſtated like the waſte, and the little ſtones abovementioned. M. *de la Hire* took ſome ſtones from the bottom of the baſon which appear'd ſpongeous, and were covered either wholly or in part

part with a kind of ſtony vegetation four or five lines high, whoſe ſtems, when viewed with a microſcope, appeared like little priſms terminated at each end by a triangular pyramid; theſe vegetations were no doubt young incruſtations, which in time would have been light like the reſt, ſeen in other parts of the quarry.

All this ſeems at firſt ſight to agree with M. *Geoffroy*'s ſyſtem, tho' upon viewing it nearer the agreement is not ſo exact——The water which had produced the incruſtations abovementioned was rain water, which in filtrating through about 10 fathoms of earth, may eaſily be ſuppoſed to have met with the cryſtalling or petrifying juice; but on the contrary the coats or incruſtations were very different from thoſe made by other petrifying waters, in their canals, which are commonly opaque and gritty, this ſeems to intimate as if there were differences in the petrifying juice.

Be this as it will it is abundantly proved that all ſtones were once a ſoft paſte, and as there are quarries almoſt every where, it follows that the ſurface of the earth was once covered with a hard mud in all thoſe places, the ſhells found in moſt quarries, are a proof that this mud was earth diluted with ſea water, and conſequently the ſea muſt have covered all thoſe places; but it could not cover them without at the ſame time overflowing all other places lower, or on a level with thoſe, nor could it cover all the places where there are quarries, and all others lower or on a level with theſe, without covering the whole ſurface of the terreſtrial globe; we do not here conſider mountains which the ſea muſt have covered likewiſe, ſince there are always quarries and frequently ſhells found therein.

The

The ſea therefore muſt have covered the whole earth, and hence it is that all the banks or beds of ſtone found in planes, are horizontal and parallel to each other. Fiſhes muſt have been the firſt inhabitants of the globe, which was not yet in a condition to bear either land animals or birds.

The difficulty is to conceive how the ſea could retire into thoſe large cavities or baſons which it now poſſeſſes, the ſolution which firſt offers itſelf, is that the globe of earth, at leaſt to a certain depth was not uniformly ſolid, but interſperſed with large cavities; the *parietes* whereof ſtood their ground for ſome time, but at length ſhuddered and broke in ſuddenly; upon this the waters ruſhing into the cavities filled them as we now ſee, and left part of the ſurface of the earth bare which in time became a proper habitation for men and other animals: the ſhell-fiſhes found in quarries are a confirmation of this opinion for beſides that none but the ſtony parts of fiſhes could have been preſerved ſo long in earth, it is certain that the generality of ſhell fiſhes are found in large numbers in certain parts of the ſea, where they remain as it were immoveable and form a kind of rocks, being utterly unable to purſue the water when it abandons them ſuddenly; it is for this laſt reaſon that we find infinitely more ſhell-fiſhes, than of bones or impreſſions of other fiſhes, which withal is a proof of a ſudden fall of the ſea-water into its channel.

At the ſame time that the cavities broke in, it is probable that other parts of the ſurface of the globe were reared up, and for the ſame reaſon theſe made the mountains which became elevated above the ſurface, with quarries ready formed therein; but the *ſtrata* of theſe quarries could not retain their former horizontal direction, unleſs the maſſes which formed the mountains happened to

riſe

riſe exactlyperpendicular to the ſurface of the earth, which could but rarely happen ; and hence as we have already obſerved in the hiſtory of 1708, the *ſtrata* of quarries are always inclined to the horizon tho' parallel to each other, for that they have not changed their poſition with regard to each other, but only with regard to the ſurface of the earth.

In ſome ſtones we meet with leaves of plants, inſects, bones of terreſtial animals and men, and ſometimes even intire ſkeletons; but all this is very rare in compariſon of fiſhes, hence it follows that after the grand revolution which bared one part of the ſurface of the earth, and rendered it habitable to terreſtial animals, other particular and leſs conſiderable revolutions enſued, which laid certain tracts under water, at the time when the earth was already ſtocked with plants and animals, theſe alſo may have produced mountains; and earthquakes and volcano's are capable of the ſame effects, and muſt even have produced greater when the ſpiracles which are now open were cloſed——But enough on this ſubject, for though all theſe conſequences ſeem to follow very naturally, it is a kind of temerity to purſue them ſo far ; it may ſuffice that the ſurface of the earth has remained quiet a long time, and we may promiſe that it ſhall remain ſo much longer, at leaſt it is extremely ſo in compariſon of thoſe of *Mars* and *Jupiter*.

II. *Of an extraordinary quantity of reſin, in a deal plank.*

A man of the firſt dignity in the church, being in *Poland*; cauſed a deal plank to be put before a chimney to keep it from ſmoaking ; this plank by little and little yielded ſo much reſin, that the whole being carefully gathered yieled five times as

 much

much as a deal plank full of its resin. There had been a great deal of resinous wood burnt in the chimney, and the plank must have imbibed a great quantity of the resin, which the fire had driven out of it, and that it drew out of it none of the earthy part in proportion to what it might have taken.

III. *Of a great inundation in the lower* Normandy.

Last *November* there was in the lower *Normandy* a terrible overflowing of the sea, which lasted until *December* 2, and extended from *Avranches* to *St. Malo.* All the low places were overflowed, the sea was extremely agitated, and there was no reflux during that whole time. There was great thunder last *November* in the lower *Normandy.* They did not remember any thing like it.

A N

ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1716.

I. *Meteorological observations made at the royal observatory, during the year* 1715; *by M.* de la Hire*.

THERE fell in

	Lin.		Lin.
Jan.	6 $\frac{3}{4}$ $\frac{1}{8}$	July	21 $\frac{1}{2}$ $\frac{1}{8}$
Feb.	6 $\frac{3}{4}$ $\frac{1}{8}$	Aug.	38 $\frac{1}{2}$ $\frac{1}{8}$
March	14 $\frac{1}{2}$ $\frac{1}{8}$	Sept.	8 $\frac{1}{4}$
April	19 $\frac{1}{4}$	Oct.	11 $\frac{1}{2}$ $\frac{1}{4}$
May	12 $\frac{1}{4}$	Novem.	24 $\frac{1}{2}$
June	30 $\frac{1}{2}$ $\frac{1}{8}$	Dec.	15 $\frac{1}{4}$

The ſum of the water of this whole year will therefore amount to 210 lines $\frac{1}{2}$, or 17 inches 6 lines $\frac{1}{2}$, which is a little leſs than 19 inches, at which we have always eſtimated the mean years. However the harveſt was good, becauſe the greateſt part of the lands of this country are moiſt, and do not want a great quantity of water; and what contributed greatly to the fertility, are the moderate rains of *March* and *April*.

The three months of *June*, *July* and *Auguſt*, have furniſhed almoſt as much water as all the o-

* February 19, 1715.

ther months together, which is pretty uſual, and without any conſiderable ſtorms.

During this whole year there were no great ſtorms. That of *July* 2 was the ſtrongeſt with a weſt wind, but it rained no more than 2 lines $\frac{1}{2}$; another of the laſt of *June* furniſhed 8 lines $\frac{1}{2}$ of water; *June* 13 there fell 14 lines $\frac{1}{2}$ of water without any ſtorm, and with a north wind. There was but little ſnow the 9th and laſt day of *December*.

The winds were very variable during this whole year.

As for the heat and cold pointed out by the thermometer, I have found mine, which I have uſed theſe 40 years, at the beginning of the year, at the loweſt at 18 parts *January* 18, and towards the end of the year at 22 parts *December* 25; it aroſe to 64 parts *July* 2, and then there was a pretty conſiderable ſtorm. It muſt be conſidered, that it uſually riſes 12 parts about 2 or 3 hours after noon, above what it is in the morning, and this elevation marks the greateſt heat of the day, wherefore if to the 64 parts, where it was in the morning of *July* 2, we add 12, we ſhall have 76 parts, which will mark the greateſt heat of this year; and if we ſubſtract the 48 from the mean ſtate, there will remain 28 parts, to which it roſe above the mean ſtate; but if from the ſame 48 parts we ſubſtract the 18 of the loweſt, there will remain 30, whence it follows, that the greateſt heat of this year has almoſt as much ſurpaſſed the mean ſtate, as the mean ſtate ſurpaſſed the cold, and as this very often happens according to my obſervations we might ſay that this part of *France* is the true middle of the temperate zone, tho' we are almoſt 4° degrees diſtant from it towards the north.

The

The barometer ſerves us to obſerve the weight of the air, but we find very conſiderable irregularities in this inſtrument, for though they are made with a great many precautions, they do not agree together. In ſome the quickſilver always riſes ſeveral lines higher than in others in the ſame place which cannot be aſcribed but to the nature of the quickſilver. That which I have uſed for a great number of years, has always its quickſilver leſs elevated by three lines than in another which is placed quite near it. There is light obſerved in both of them, when the quickſilver is moved in its tube, that wherein the quickſilver is the higheſt, was the firſt wherein the light was firſt obſerved.

The quickſilver roſe in my barometer to 28 inches 3 lines $\frac{1}{2}$ at the higheſt, *January* 22, there being a moderate S. E. wind, and a great fog, but fogs are not obſerved to have any effect upon it. *December* 1, it was alſo at 28 inches, and almoſt 3 lines, with a moderate N. wind; and the 6th of the ſame month it fell to 26 inches 9 lines $\frac{1}{2}$ with a moderate S S W. wind; thus the difference of the heights of the quickſilver this year was 1 inch 6 lines, which is the greateſt variation of height in this country, and it often happens.

This inſtrument may ſerve to foretell pretty nearly the alterations of the weather, which happen from day to day, and yet we cannot be very certain of it, but in general when the quickſilver is low, there ſhould be rain, and when it is high there ſhould be fair weather, though the air is then much heavier than when it is low. I am perſuaded that the fair or rainy weather does not depend upon the weight or lightneſs of the air, but that it comes only from the wind, and I do not mean it of the wind in general, but of thoſe winds which come from far, either from the utmoſt north, or

utmoſt ſouth, and not of thoſe which are engendered on the ſurface of the earth; for the ſun raiſing the vapours more in the ſouthern than in the northern countries, the ſouthern winds muſt give us rain more often than the northern. And as we know by all the obſervations that have been made towards the north, that the atmoſphere is higher there than towards the equator, it muſt happen that the winds which come from the north, will raiſe the atmoſphere in our temperate zone higher than uſual, and conſequently the quickſilver will riſe by the greater weight of the atmoſphere, and the air will become clear becauſe of the north wind. It will be contrary with regard to the winds which come from the ſouth, into theſe countries. What I have juſt ſaid for our northern temperate zone, muſt be extended in like manner to the other, which is ſouthern.

It is commonly obſerved, that towards the middle of the ſpring the wind is pretty cold, though it comes from the ſouth, where the earth is very much heated by the preſence of the ſun, and we ſay it is not yet ſufficiently heated to warm the air which touches it, and is brought to us by the wind, but I think we may yet give another reaſon for it; for at that time the lands, from whence this wind comes, are covered with herbs and green trees, the leaves of which are not eaſily heated by the ſun which touches them, and conſequently cannot heat the air which ſurrounds them, contrary to what muſt happen when theſe herbs are dried, and the ſun immediately heats the earth or ſand, which receive a very great impreſſion from it. I do not ſpeak of the waters, for it is known that they receive but little impreſſion from the preſence of the ſun.

We

We have examined the declination of the needle *December* 30, with three different needles and of different conſtruction, 2 of which were 8 inches long, one of theſe needles is that which I have uſed for a great number of years, and we have found it to be 11° 10′, in the ſame place and in the ſame manner as the preceeding years. This declination is a little leſs than that of laſt year. The third needle is 13 inches ½ long, and it has given us the ſame declination as the two others, 11° 10′. Theſe three needles are very light and moveable. My old needle of 8 inches is a ſteel wire ending in two ſlender points, the other, of the ſame length, is in form of a flatted ſpindle, as ſeveral have been lately made.

The third, which is 13 inches ½, is a little plate of ſteel, very fine and thin, but placed in a box after a quite new manner, wherefore I thought proper to give a deſcription of it, which is the ſubject of the next article.

II. *Of the conſtruction of the compaſſes uſed to obſerve the declination of the magnetical needle; by M.* de la Hire.

The box uſed in theſe ſorts of compaſſes muſt be of a ſquare figure, or elſe oblong, the two ſides of which, that are to be turned toward the north, muſt be exactly parallel, and exactly ſquare with the bottom of the box. The matter of this box is uſually braſs or wood, very cloſe, and not ſubject to warp either by moiſture or dryneſs; thoſe which are of braſs, or in which there are any pieces of braſs, are ſubject to errors, eſpecially if the braſs has been melted, for there are often ſome grains of iron in it, which turn the needle from its true direction.

direction. Thofe which were are of wood warp very eafily, and as they are made of feveral pieces they very often unglue, and it may be fufpected that there are fome grains of iron in the glue with which all the pieces are joyned. Wherefore to avoid thefe accidents, I have thought of making thefe boxes of white marble, or of *pierre de liais**, which is almoft equal to it in hardnefs.

On the bottom of this box muft be drawn both on the infide and outfide a right line, according to its length, which divides its breadth into two equal parts, or if the box was fquare, thefe lines muft be parallel to the fides of the box, which is to be ufed in the obfervation to direct them according to the meridian line.

To draw thefe lines true, and to prove whether the box is of equal breadth every where, we muft make a caliper of tin or pafte-board, which embraces the fides of the box, and may go down to the bottom on the infide refting on the fides of the box. Divide this caliper length ways into two equal parts by a fmall line, and the extremity of this line will mark upon the bottom of the box, the middle of the breadth of the bottom of the box, and underneath the box the *trufquin* will do the fame office. This *trufquin* is an inftrument of the joiners.

Afterwards having divided the length of the line drawn on the bottom of the box into two equal parts, make a little hole in the point of divifion, which muft anfwer to the middle of the line under the box; this hole will ferve to receive the pivot, which is to fuftain the cap of the needle. This pivot muft be of brafs, not of fteel, and muft

* A very hard fort of ftone, taken out of the quarries of *Arcueil*, near *Paris*.

diminifh

diminiſh from its baſe to its point, which muſt be very fine, and placed exactly perpendicular on the bottom of the box, and anſwer to the point which equally biſects the line drawn through the middle of the box. This pivot muſt be ſo firm as not to vary by the motion of the needle, when the compaſs is removed, the caliper being applied acroſs the box, will ſhew whether the point of the pivot is true in its place. It muſt be obſerved, that the pivot ſhould be of a proper height, to leave the needle at liberty to move from one ſide to the other, without being too much hindered by the bottom of the box, nor by the glaſs with which the box is covered.

On the inſide of the box and towards the extremities of its length, if it is not ſquare, faſten two equal arches of a circle, which muſt be divided into their degrees, and into their leaſt parts poſſible. The inner *radius* of theſe arches muſt be equal, or as little as poſſible greater than the half of the length of the needle, that by turning upon its pivot, it may almoſt touch with its points the inner edge of theſe arches, to ſhew exactly the quantity of the declination of the animated needle.

The matter upon which theſe arches are to be drawn, muſt not be of braſs, for fear, as I ſaid before, there ſhould be a grain of iron in it, but rather of pewter, ivory, or fine paſtboard. Theſe arches muſt be a little elevated above the bottom of the box, being placed on two brackets of wood, to the height of the needle, and theſe brackets muſt be ſtrongly fixed in the bottom of the box. The arches are faſtened upon the brackets, when the right line which paſſes through their middle, or thro' the points of o of their diviſion, paſſes alſo through the point of the pivot, and when it is exactly parallel

parallel to the ſides of the box, which will be eaſi-ly done, by making uſe of the caliper already em-ployed, from which a part of its height may be retrenched, ſince we have no more to do than to go juſt to the top of the arches; for this caliper being applied to the breadth of the box, and em-bracing its ſides, the line which is drawn in its middle, and which is parallel to the ſides of the box, muſt agree with the points of o of the arches and with the point of the pivot, as has already been done.

All the needles of the compaſſes ſhould be of tempered ſteel, but as light as their length can poſ-ſibly admit. They are made of different figures, and the moſt common are in form of a flatted ar-row, and the point of it which repreſents the head muſt turn to the north, and the oppoſite point to the ſouth. The right line, which goes from one point to the other, muſt paſs through the bottom of the cap, for which reaſon we may bend the two branch-es a very little upwards, but we cannot be ſure that the two points agree with the bottom of the cap but by trying.

We ſuppoſe in the firſt place, that the needle is well prepared, and well poiſed upon its pivot. When the two arches of a circle are placed and faſtened in the box, care muſt be taken that the points of o of their diviſion and the point of the pivot be in a right line; wherefore if the needle is well prepared when it is put upon its pivot, its two points muſt agree exactly with the ſame two points of diviſion of the arches, and if the points do not agree with them, we need only gently bend one of the branches upon the flat to turn the point where it is neceſſary. This practice will ſerve for other needles of any figure whatſoever.

When

When these sorts of needles are very long, as a foot or more, they become very heavy, and are subject to stop out of their true position; wherefore some have been made of these lengths, with very thin branches, and bearing at their extremities two pretty thick pieces of steel ending in a fine point. These two bits of steel being animated with the needle, are like two loadstones situated in those places. When these needles are in motion, they make a great many vibrations up and down by the great flexibility of the spring of the branches. But these two bits of steel being therefore like two loadstones joined by the branches of the needle, one might suspect that the direction of the magnetical matter in one of these loadstones, would not be just the same as that of the other; and that there might be one compounded out of them far from the true one, much as it happens when we put upon the glass of a compass another animated needle placed on its pivot, and exactly over that within the box; for we see that these two needles turn several degrees, one of them one way and the other to the other, and that they place themselves one above the other contrary to their poles, as it happens to a loadstone that has been cut in two, according to its poles.

Other needles have been made in form of a shuttle, flat, and pointed at both ends, with the middle perforated to receive the cap; but I have observed, that they always are very heavy; if they are pretty long, it is true they may contain a great quantity of magnetical matter, but yet, as has been said, they are very sluggish.

In the last place I am persuaded by experience, that the best of all the needles, are those which are formed of a steel wire, very strait, and a little flatted and pointed at the two ends, and a good

deal extended in the middle, to be perforated in order to receive the cap. But as this cap, which is of braſs, is always heavy, I have thought proper to take off a great part of it at the bottom, and from the ſide of the branches of the needle, and to leave it but about the third part of its height toward the point ; by this means the needle becomes very light, and cannot get out of the pivot, for the vibrations of the needle, which are made according to its length, are too ſhort towards the pivot to get away from it, contrary to thoſe which are made ſidewiſe ; but in this place the cap is not voided.

To give the laſt perfection of the box, we muſt make a little ledge on the top of the inſide of the box, to ſupport a glaſs, which muſt be a very little raiſed above the top of the cap, which has two little wings towards its point, that are perpendicular to the length of the needle. Theſe wings ſerve to hinder the needle from getting off its pivot, when the box is removed.

Great care muſt be taken to ſtop the clefts between the edges of the glaſs and the ledge, for fear the wind ſhould get in, which would ſhake the needle. I have found, that to remedy this inconvenience, we may glue upon the ledge ſome little ſlips of fine cloth for the glaſs to reſt upon, and by this means the wind cannot get into the box.

When we obſerve the declination of the needle, it is always proper to turn the box different ways, to ſee whether the ſame declination is found on both ſides ; and if the box is long, and the needle cannot make an entire turn, we may change the poſition of the needle in the box.

My principal deſign was to ſpeak only of the great compaſſes, upon which one may eaſily ſee the degrees and their parts, and not of thoſe which are

are uſed at ſea, which are ſo clumſy, that we may juſtly wonder, how they can truſt to them in the guiding of a ſhip; but we have nothing better, or more convenient.

III. *A method of ſhooting bombs to advantage, by M.* de Reſſons*; *tranſlated by Mr.* Chambers.

Tho' theory joined with practice be the ſureſt way of attaining the perfection of an art, yet experience has convinced me, that theory is of very little ſervice in the uſe of mortars.

In M. *Blondel*'s book we have the diſtance of the parabolic lines, for the different degrees of elevation, deſcribed with great exactneſs; but by the practice it appears, that there is no theory in the effects of gun-powder; for having applied myſelf to point mortars agreeably to his calculations, I could never fix any foundation on their footing.

I will not however advance, that if the bombs were all of an equal weight, if the powder were always diſpoſed alike, and the platform ſo firm as never to give way, the theory might not be uſed to good purpoſe, but ſuch a multitude of irregularities ariſe both in the manner of charging mortars, the different weights of bombs, and the different qualities of gunpowder, that the moſt experienced gunner cannot anſwer for throwing three bombs running with exactneſs, unleſs he uſe the following expedients, which long practice has taught me.

I begin with ſhewing all the irregularities, which I divide into 3 claſſes; 8 whereof lie in the

* March 24, 1716.

bomb, 13 in the mortar, and 4 in the gun-powder.

1ſt. They differ from each other in weight, for that being all caſt ſeparately in moulds, ſome alteration always happens either in re-heating thoſe moulds, or the core in the middle which is ſometimes bigger, and ſometimes leſs, or by the ſows being run hotter, and conſequently being more liquid, or cooler, and more denſe, which depends on the moiſture or dryneſs of the weather, by all which, bombs out of the ſame furnace, and the ſame hands, uſually differ 5 pound in weight from each other.

2dly. Becauſe the beads which ſuſtain the core are not always equal, but ſeveral lines longer or ſhorter than each other; whence the bomb becomes thicker of metal on one ſide than the other, which gives it a biaſs in the air towards the heavieſt ſide.

3dly. The different ſituation of the ears of the bomb, which the workman only places by his eye, but which being a few lines nearer, or farther off the centre of the mouth incommode it in its paſſage by the reſiſtance of the air, which is greater or leſs according to the poſition of the ears.

4thly. By reaſon there frequently prove bliſters or cavities in the metal, which change the *equilibrium* of the bomb ; and the air entering ſuch cavities when they chance to be open, (as may be known by the hiſſing it makes) the motion of the bomb is retarded hereby.

5thly. Becauſe the moulds frequently chap as they dry, and thus occaſion ſeams and inequalities on the ſurface of the bomb.

6thly. Becauſe the core is ſometimes ſituate too much forwards or backwards; and thus putting the

the cavity out of its place, changes the proportion and *equilibrium* of the bomb.

7thly. Becaufe the fuzees ufed in bombs cannot be made with juft one inch projecture, as it were to be defired; but fome ftand out an inch and $\frac{1}{2}$, or 2 inches, and others only $\frac{1}{2}$, or $\frac{2}{3}$ of an inch, whence according to their different lengths, they have different effects on the air no ways to be prevented; for that when a fufee when driven to its pitch muft be driven no further, for fear of burfting, as I have frequently known it do, and hereby burft the bomb in the mortar as foon as fired.

8thly. That the bomb has its mouth frequently a-crofs, whence the fufee being obliquely placed, retards its courfe in the air, and makes it wabble.

I could mention numerous other inconveniences, but having touched on the principal, I fhall pafs them over, and proceed to the defects arifing from the mortar.

1ft. How carefully foever the mortar be pointed by the feveral degrees, a good number of throws are always made, ere the proper one be found.

2dly. When it is found, 'tis very difficult to replace it in the fame point, either upon the platform's yielding, or the mortars fhaking to the right or left in the holes of its trunnions.

3dly. Suppofing it exactly pointed every time, when the powder is put in the chamber, they lay a cloth over to prevent its communicating with the earth; and this cloth frequently makes vacant fpaces, or the powder is more on one fide than the other.

4thly. Being obliged to cram earth in to fill the chamber more compleatly, it fometimes proves

moiſt, and ſometimes dry, and thus has different effects on the powder.

5thly. Over this earth they always lay a wooden tampion, which they beat down with a rammer, but beat it unequally, which makes a conſiderable alteration.

6thly. The tampions are frequently either too big, or too little; if too big, they do not enter far enough, but leave an empty ſpace within the chamber; and if too little, they ſink too deep; and thus confine the charge, either of which is a great inconvenience.

7thly. The tampions are frequently driven more on one ſide than the other, ſo that ſtriking the bomb out of its centre, they make faulty ſhots.

8thly. After the bomb is placed in the mortar, they are obliged to fill its muſsle with earth, which they beat in with the edge of the rammer, and ſometimes more of it, and ſometimes leſs.

9thly. The bomb is frequently found either too deep or too ſhallow in the mortar, by reaſon of the different thickneſſes of its bed, occaſioned by the more or leſs earth put therein, which alſo occaſions great errors.

10thly. The platforms ſhrink and recoil in the exploſion.

11thly. They frequently incline more one way than the other, which throws the bomb either to the right or left accordingly.

12thly. One trunnion has frequently more play and liberty than another, which makes the mortar throw falſe.

13thly. As the mortar heats, the more you ſhoot, the more ſkill is required to leſſen the powder in proportion.

These are the chief defects which arise from the mortar: as to the powder,

1st. The gun-powder in one and the same barrel is rarely equal, or if it were, when that barrel was spent, and another opened, there will be a notable difference, either by the one's having been laid over the other, to the sun or rain, or by the others having lain on the earth, and attracted moisture.

2dly. Tho' the powder were ever so equal, errors would arise from its disposition and arrangement in the mortar, being sometimes too close, and at other times too open.

3dly. The grain of the powder being unequal, when it is a little too big, the fire will be better communicated thro' the space, than when smaller and closer.

4thly. The weather makes a change in the powder, which is weaker in moist weather, and stronger when dry.

I might enumerate several more defects, but those already mentioned suffice to prove that how justly soever a mortar be pointed according to the rules, 'twill be difficult, so many inconveniences which long practice has taught me to do, and which I have performed with success in the sieges of *Nice*, *Algiers*, *Gènoa*, *Tripoli*, *Roses*, *Palamos*, *Barcelona*, *Alicant*, and numerous other places bombarded under my care.

To remedy the defect of bombs ere we proceed to charge them, we must range them with the mouth upwards and the breech downwards, as perpendiculary as possible, so that by viewing them one after another it may be easily perceived whether any of their mouths be awry, which will denote them thicker in metal on one side than on the other, all which are to be thrown by; and the like

like muſt be done by thoſe whoſe ears are diſproportioned, or which have any conſiderable bliſters in the metal, reſerving all theſe faulty bombs 'till there be occaſion of throwing into towns, where if they miſs one part they may do execution in another.

Having made choice of the better, a number of the beſt of theſe muſt be weighed and reſerved for ſhots of conſequence, dividing them into ſeveral lots, and throwing all thoſe from 125 pounds to 130 pounds into one lot; thoſe from 130 pounds to 135 pounds into another lot; and thoſe from 135 pounds to 140 pounds into another; and ſo of the reſt: then chuſing for each bomb a fuſee, which may only ſtand an inch out of the mouth when the bomb is charged, and the fuſee driven full to its place, the bombs are to be laid by in their lots to be in readineſs for throwing upon any important place, as a magazine of powder, a retrenchment, or the like. The reaſon is, that having ſuch bombs nearly of an equal weight, the quantity of powder may be adjuſted to the lot they are taken from; thus much for the bomb.

As to the powder, to have it as equal as poſſible an eſtimate muſt be made of the number of bombs to be thrown in a day or night, and the quantity of powder requiſite thereto to be computed. For an inſtance finding 200 bombs like to be thrown in a night, at 6 pounds of gunpowder each, 1200 pounds of powder muſt be poured on a large cloth and well ſtirred and mixed together, after which it may be put up in barrels again, and will be found as equal as military expedition will admit off.

As to the mortar, care muſt be taken to make two platforms, one on either ſide for each mortar, and

and when one of them has ſhrunk by a multitude of ſhots, the mortar muſt be placed on the other; while the former is refitting to charge the mortar, it muſt be ſet upright on its trunnions, and the powder poured into its chamber, by meaſure not by weight, ranging it as even as poſſibly with the hand, and then ſpreading a dry cloth thereon, cut of the proper ſize ſo as there may neither be too much nor too little, the chamber then to be filled up with earth, which is to be well beat down with the hand, then without any wooden tampion between half an inch thick of earth to be laid thereon as a bed for the bomb, it remains now to put the bomb in the mortar with its mouth in the middle of the barrel, taking care that it touch not the metal on either ſide, which may be prevented by caſing it all around with earth.

The mortar being thus charged, if the bomb happen by accident to have only one ear, the other being broke off in the charging or diſcharging, care muſt be taken to break off the ear remaining, with a wooden mallet, otherwiſe it will fly awry.

Every thing thus prepared the mortar is to be gently let down, and pointed by the quadrant to the degree found moſt convenient, aim may be taken by a plummet which dividing the mortar exactly into two, the ſight is to be levelled over it to the object as exactly as poſſible, but in all this practice is of more effect than theory.

IV. *On the longitude of the ſtreights of* Magellan, *by M.* Deliſle*.

We know it is of great importance for navigation to be well aſſured of the longitude of places frequented by our ſhips. And it muſt be allowed to be of great advantage to make uſe of all opportunities of rectifying what knowledge we have of it, not only by aſtronomical obſervations, but alſo by other ways.

In effect the obſervations made by two navigators on board the *Saint Louis*, which I have related in the memoirs of 1710, ſhew 300 leagues of error in the map of *Pieter Goos*, in the diſtance of the ſtreights of *Magellan*, from the rocks or iſles of *Triſtan de Cunha*; that this error would have cauſed the loſs of the ſhip, but that happily the rocks were perceived in the day-time, and that this great difference between the eſtimation of the officers and a map which all navigators make uſe of, had made them fancy theſe iſles to be a new diſcovery.

I have advanced in the ſame memoir, that in the beſt maps the diſtance was ſtill too great by 170 leagues, and among others in Dr. *Halley*'s chart of the variations, in which the mouth of the *Rio Gallega* at the eaſtern part of the ſtraights of *Magellan*, was marked 10° more to the weſt than it ought. I have ſupported this correction not only by the eſtimation of theſe gentlemen, but alſo by an obſervation made by F. *Maſcardi*, in the valley of *Bucalene* in *Chili*, the diſtance of which from the *Rio Gallega* being known, I concluded its longitude with regard to *Paris*, and its diſtance from the *Cape of Good Hope*.

* April 4, 1716.

Dr.

Dr. *Halley* in the philoſophical tranſactions*, does not allow of this correction, and ſays he can by no means grant an error of 10°, to be poſſible in his longitude of the *Magellan* ſtreights. He ſays, the 1350 leagues, which the ſhip *St. Louis* made from the ſtraights of *Magellan* to the *Cape of Good Hope*, confirms inſtead of invalidating what he has laid down concerning the ſtreights of *Magellan*, and the *Cape of Good Hope*, but it muſt be obſerved, that tho' the courſe of this ſhip was in general E. N. E. yet it could not exactly follow this rhumb, during ſo long a traverſe, turning ſometimes to the N. and at other times more to the E. and that the eſtimation of theſe gentlemen makes but 85° 50′ between theſe two lands, which comes, as I have ſaid, to 1° ½ nearly, as reſults from the obſervations of F. *Marſcardi.*

Dr. *Halley* gives the reaſons which determined him to aſſign this longitude to the ſtraights of *Magellan*; and they are, firſt, an eclipſe of *Sept.* 18, 1670, obſerved by Capt. *John Wood* in port *St. Julian*, at juſt 8 at night; and at 14ʰ 22′ at *Dantzick*, by M. *Hevelius*; by which the longitude is known with regard to *London*, whence he concludes the difference of the meridians between *London* and this port to be 76°. Second, the eſtimation of Capt. *Strong*, whoſe journal he has in his cuſtody, which makes 45° of longitude between the ſtraights of *Magellan* and the iſland of *Trinidada*, of which Dr. *Halley* ſays he knows the longitude with regard to *London*. Hence he concludes that the eaſtern part of the ſtraights of *Magellan* is 75° more weſterly than *London*, as he has marked it in his chart. Laſtly that the currents carry the ſhips weſtward towards the

* N°. 341. Jones's Abridgement, Vol. IV. Page 454.

coaſt of *America*, which makes the land appear more eaſterly then it really is.

To theſe reaſons I oppoſe, firſt, that the obſervation made by F. *Maſcardi*, which F. *Riccioli* ſays is exact, ſeems preferable to that which Dr. *Halley* relates from Capt. *Wood*, though he may be a good ſailor.

Second, that the currents, which ſet to the W. towards the coaſts of *America*, reach no further than to the 30th degree of ſouthern latitude, and that there are places, where the current is quite contrary, ſetting to the E. as I have related in the memoirs of 1710, on occaſion of the voyage of M. *Bigot de la Canté.*

Laſtly, that F. *Feuilleé* having exactly obſerved, in 1709, ſeveral immerſions of the firſt ſatellite of *Jupiter*, at the *Conception* and at *Valparaiſo*, towns of *Chili* near *Bacelene*, theſe obſervations being compared with thoſe which were made at the ſame time at *Paris*, not only confirm in general that of F. *Maſcardi*, but authoriſe ſtill more the longitude which I had aſſigned to the ſtraights of *Magellan*; whereas, according to Dr. *Halley*'s hypotheſis, the eaſtern parts of theſe ſtreights being ſuppoſed 75° more weſterly than *London*, and conſequently 77 ½ more than *Paris*, as F. *Feuilleé* finds only 75° ½ between *Paris*, and the *Conception*; it ſhould follow from thence, that the entrance of the ſtraights of *Magellan*, on the ſide of the north ſea, would be more weſterly by 2°, than the *Conception*, on the coaſts of the ſouth ſea, which is contrary to all probability.

V.

V. *Obſervations on a northern light; by M.* Maraldi*.

We have obſerved a rare and curious phœnomenon, which appeared in *April* 1716. We began to ſee it on the 11th at $10^h \frac{1}{2}$ P. M. two hours after the twilight was entirely gone.

It was a great whiteiſh light expanded along the horizon, from N.W. to N. It began where the ſun ſets in ſummer below the weſtern foot of *Auriga*, then ſpread over the weſtern leg of *Perſeus*, and paſſing between the conſtellation of *Andromeda* and that of *Caſſiopea*, it ended in that part of the heaven, which was below the head of *Cepheus*. Thus far did this light extend from W to N which was about 80°.

Its breadth was terminated on one ſide by the horizon, whence it ſeemed to come out, and roſe to the height of 7°, except toward the two extremities, where it was leſs broad, its brightneſs was equally ſpread on all ſides, and it grew weaker only toward its upper part.

The ſky was ſo bright, eſpecially in this part of the horizon, and this light was ſo clear, that we could ſee thro' it with the naked eye, the principal ſtars of *Perſeus*, *Andromeda*, *Caſſiopea*. The ſtar, which is in the weſtern knee of *Perſeus*, was about 2° lower than the upper extremity of the light; the fine ſtar in the head of *Meduſa* appeared in the middle of its breadth; we ſaw alſo towards its upper bound, the moſt northern ſmall ſtars of *Andromeda*, and the moſt ſouthern of *Caſſiopea*.

We perceived, that the ſtars of the head of *Meduſa*, and of the knee of *Perſeus* were leſs immerged in this light at the beginning of its appear-

* April 22, 1716.

ance

ance than afterwards, eſpecially the ſtar of *Perſeus*, which made at the ſame time more variation in height than the reſt, whilſt the light was equally broad, and paſſed always through the ſame ſtars near the meridian, which did not vary ſenſibly in diſtance with regard to the horizon, which ſhews, that this light did not partake of the motion of the *primum mobile*; and conſequently that it was not celeſtial, but rather fixed to our atmoſphere, and that it is different from the light diſcovered on the zodiac by M. *Caſſini*, which partakes of the motion of the *primum mobile*, and of the proper motion of the ſun.

Beſides the conſtant and uniform light, which was like the day-break, but more bright and whitiſh, we ſaw from time to time columns of a ſomething more vivid light, which had the appearance of the tails of comets. Theſe columns began to appear at the horizon, and being driven as it were upwards, roſe a little above the upper extremity of the light. They ſeemed to imitate ſpouts of water or fuſeés, and one might ſay there were ſo many ſpouts of light.

We ſaw ſeveral of them at once, which traverſed in different places the horizontal light, and riſing higher, rendered it unequal in its upper part. They were about 2° broad, roſe to the height of 7 or 8, and were viſible but about $\frac{1}{4}$ of a minute, or $\frac{1}{2}$ a minute at moſt. When theſe columns had diſappeared, we were 8 or 10 minutes without having any, after which there appeared ſeveral others like the firſt, in different parts of the light: thus this appearance began ſeveral times in the ſpace of an hour, and continued till half an hour after 11 at night.

After 11 hours $\frac{1}{2}$, we ſaw no more of theſe perpendicular columns, and the horizontal light, which

which had till then preferved its brightnefs, gradually diminifhed, whether it was effaced by the prefence of the moon, which rofe that day at 11 hours $\frac{1}{4}$, that is, $\frac{1}{4}$ of an hour before the columns difappeared, and the light faded; or the matter, which was the origin of it, was not in fuch abundance as at the beginning.

In whatfoever manner this happened, the light diminifhed, fo that a little after midnight it was no longer fenfible.

The next day, which was the 12th, the fky being clear, we faw a track of light like that of the preceding day, fpread underneath the conftellation of *Caffiopea*, but this light was faint, and difappeared in a little time. We faw but once thefe luminous columns, which rofe perpendicularly to the horizon. That day and night there was a ftrong fouth-eaft wind.

April 13, at 8 hours $\frac{3}{4}$ P.M. the twilight being ended, the fky beautiful, and the air calm, we began to fee at the horizon below the ftars of *Caffiopea* a faint light, which was ftill in the fame ftate at 10 hours. But at 10 hours $\frac{1}{2}$, it was very much augmented, both in magnitude and brightnefs. It was much larger and finer than the day before, but a little lefs bright than the 12th, efpecially toward the extremities. Its fituation and extent were the fame as the firft day, and it remained in its brightnefs the fpace of $\frac{1}{2}$ an hour. Whilft we were confidering the bounds of this light, we faw appear towards the moft eaftern extremity of it, one of thofe columns of light, which by a fucceffive motion from north-eaft to north-weft, ran in a few minutes thro' its whole extent, and ended at the weftern extremity. Afterwards the horizontal light began to fade, and entirely difappeared about $\frac{1}{2}$ an hour after 11.

The

The clouds having interrupted the obſervations 2 days ſucceſſively, the light did not appear after the ſky was cleared. We have been informed by a relation ſent to M. *de Valincour*, that at *Dieppe*, *April* 11, at 10 hours ½ P. M. they obſerved at the horizon toward the weſt, a cloud, which extending toward the north, and riſing in the ſpace of an hour to the height of 35°, formed itſelf in manner of a globe, which afterwards became red, and roſe again perpendicularly, after which it emitted a flame, which laſted about ¼ of an hour. This globe afterwards deſcended near the horizon, where it diſperſed, and the ſky being covered at midnight, the *phænomenon* diſappeared.

This obſervation of *Dieppe* was made the ſame night with ours, and they agree in ſome circumſtances, for the *phænomenon* appeared to the north, and it laſted in both places till midnight. But there are other circumſtances, in which the *phænomena* do not agree. At *Paris* we began to ſee the light at 10 hours ½; at *Dieppe* at the ſame hour they began to ſee the cloud to the weſt, and this cloud did not appear red, till it came toward the north at 11 hours ½: thus at *Dieppe* the light did not appear till an hour after the firſt obſervation at *Paris*.

We ſaw for an hour together ſeveral luminous columns proceed from the light, whereas in the account from *Dieppe*, they mention only a flame, which laſted but ¼ of an hour.

Theſe different circumſtances ſhew, that it is not the ſame *phænomenon*, that appeared at the ſame time in theſe two cities, but two different ones, which may be cauſed by matter of the ſame nature expanded in the atmoſphere of theſe two different cities.

In

In *England*, and in ſome of the weſtern cities of *France*, *March* the 17th, 1716, there was ſeen a great *phænomenon*, which ſeemed to have ſome relation to that which we have obſerved.

At *Newark* in *Nottinghamſhire*, they ſaw about an hour after ſun-ſet toward the N. W. 2 pretty dark clouds, but little diſtant from each other, and elevated from 20 to 25° above the horizon, from each of theſe clouds proceeded with great quickneſs a light like a tail, which reſembled thoſe rays that proceed from the clouds when the ſun is near the horizon. Theſe rays extended ſo far as to cover a part of the heaven from N. W. to N. and did not hinder the ſtars from being ſeen through them, though a little more faintly, juſt as they are ſeen through very thin clouds. In the reſt of the heaven the ſtars were as bright, as in the cleareſt froſty nights in the abſence of the moon.

At 9 at night theſe ſtreams of light diminiſhed until 10, when they began again, and continued till 11h ½. The light made by theſe meteors was ſo great, that they could ſee to read capital letters by it.

At *London*, towards the N. E. the horizon ſeemed to be loaded with very black and thick vapours, in the middle of which was ſeen a body of reddiſh light, which flaſhed from time to time, and darted its rays like ſtreamers towards ſeveral parts of the heavens. Theſe ſtreamers ſpread with great rapidity, and formed in the air waves of a luminous ſmoak, which went away and came again generally with the ſame figure and direction. The luminous ſmoak was ſo tranſparent, that the ſtars were ſeen through, and it was ſo bright, that

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the houſes were diſtinguiſhed by it, it perfectly imitated the brightneſs of the moon*.

According to an account ſent to M. *de Valincour*, from *Breſt*, *March* 17, the ſky being very fine and clear, there was obſerved about 7 in the evening a ſort of rainbow, of an uniform white colour, and very bright, it was ſituated toward the S. and poſſeſſed a great compaſs of the heavens from E. to W. From the E. it began near the lion's tail, it afterwards ſpread over the conſtellation of the twins, which was in the middle of the heavens, where it was 3° broad, and continuing over the *Pleiades*, it terminated alſo in a point at the head of *Aries*.

It appeared like a very white cloud, and penetrated by ſome light ſo as to produce a ſort of day-light, without hindering the ſtars from being ſeen through.

This arch being gradually diſſipated, about 9 o' clock, they ſaw toward the N. near the horizon a light which reſembled a fine day break, and extended from N. W. to N. N. E. From this light proceeded continually ſome very white and bright rays, which caſt upon the earth a ſort of day like the breaking of the day in ſummer. Theſe rays were parallel to one another, and roſe to 48 or 50° in height; they frequently appeared and diſappeared, but after diſappearing before they appeared again, there came out of the horizontal light ſome duſky vapours in form of waves parallel to the horizon, which roſe with an extreme quickneſs up to the zenith, where they diſappeared.

* The reader may find an account of the appearance of theſe lights in *England*, collected by the late learned Dr. *Halley*, from ſeveral papers laid before the royal ſociety, and his own obſervation. *Philoſ. Tranſ.* № 347. Jones's Abridgment, Vol. IV. p. 138.

ed. This alternative lafted till 11 a clock, and during this whole time they faw very clearly thro' thefe vapours and the horizontal light even the fmalleft ftars.

About 11 there appeared in the N. a ftronger light than the former, which fpread very white rays all about. At 11 ½ the ftars were obfcured, and a cloud which covered the heavens, made the light difappear.

They then ceafed to obferve, thinking the *phænomenon* over; but the next day the fifhermen related, that at two in the morning, the fky being cleared, the light appeared again more ftrong, fending forth very bright rays.

The fame day at *Dieppe*, two leagues from the fea, between 7 and 8 in the evening, they faw as it were hairy comets rifing from the fea, which lafted till 9, when there appeared a furprifing brightnefs towards the *Englifh* coaft. In this brightnefs they faw flames arife, which mounted up to the clouds like great fires.

At *Rouen*, when it was almoft dark, they faw the northern horizon illuminated by very white and bright clouds, they began at the eaft of *Lyra*, and extended 25 or 30° beyond toward the fummer fetting of the fun. When they were arrived at their greateft brightnefs, there proceeded from them rays of light, which darted fome more others lefs; and fome rofe as high as *Caffiopea*; thefe rays appeared for fome minutes, and then were diffipated; this *phænomenon* happened feveral times, but always between the fummer fetting and the pole. This part of the heavens, from the fummer fetting to the eaft of *Lyra*, was covered with a white vapour, through which they difcovered the leaft ftars. This continued till 8 o' clock, when this whitenefs covered the whole heaven,

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and they ſaw no more rays formed, though the north ſtill remained illuminated. They afterward perceived, that under the pole at the horizon, turning a little towards the E. there was a brightneſs formed, which rapidly became very big. From this brightneſs proceeded very thin tranſparent vapours, which roſe very rapidly, like great ſtreams, which at firſt had but a little light, but became luminous as faſt as they roſe; and when they were riſen to almoſt half the height of the pole, they ſtopped, and rolling upon one another formed a very luminous thickneſs, which afterwards diſperſed; this happened 12 or 15 times in leſs than ½ an hour, and at 11 theſe rays ceaſed, and the light of the horizon diminiſhed. This obſervation, which ſeems well circumſtanced, was ſent to M. *de Fontenelle*.

In the account from *Dieppe*, there is mention only of a horizontal light and rays proceeding from the ſea, which ſeems to be the *Engliſh phænomenon*. But at *Rouen*, beſide the horizontal light, which was ſeen to the N.W. as in *England*, they obſerved other rays which riſing about 25°, came out of the clouds, which were at the horizon under the pole, turning towards the N. E. whence there is room to believe, that this laſt light ſeen toward the N. N. E. cannot be the ſame with the *Engliſh* one which was ſeen to the N. W.

On the ſame night alſo, *March* 17, there was a great light ſeen on the coaſts of *Languedoc*, according to the information, which was carefully given of it, by the lieutenant of the admiralty of *Agde*, and ſent to the *Abbe Bignon*. Several maſters of barks, who were fiſhing, related, that about 9 or 10 they perceived to the W. of the pool of *Vendrés*, a great brightneſs, in colour almoſt like that which we ſee at ſun-riſing, and even redder

der, which was divided into columns, ſome brighter than others, and ſeen by thoſe who were nearer land, between the pool of *Vendrés* and the little village to the W. called *Grouiſſa*; this brightneſs was ſo great that they plainly diſtinguiſhed *Cape St. Pierre*, 3 miles diſtant from the pool toward the W. but the fiſhermen, who were further off, did not ſee this light either ſo fine or ſo large; it was ſeen for an hour, and afterwards ſcattered toward the S. three of theſe barks having been obliged by a ſquall of wind to run to the cape of *Quiers*, they found that the inhabitants of this cape had alſo ſeen this light upon the coaſts of *Languedoc*, and took it to be a fire.

As far as we can judge by this account, compared with a particular and exact chart of theſe coaſts, it appears, that with regard to thoſe who were in the parallel of the pool of *Vendrés*, and alſo of that of cape *St. Pierre*, the light appeared from the N. W. to the S. W. whereas in the northern parts of *France*, it was ſeen between the N. W. and N. E.

The *Abbé Bignon* has received another obſerſervation of a particular *phænomenon*, made by the prince of *Moldavia* at *Solnin* in the *Ukraine*, after the following manner.

March 15, 1716, at 4 in the night, toward the N. E. and at the height where the ſun uſually arrives 2 hours after it is riſen, they ſaw a ſort of long and very thin light, which afterwards extended conſiderably in form of a column, the baſe of which was crinkled, and the upper part in a point like a lance, it was of the colour of fire, and its breadth was diſtinguiſhed by ſeveral white flutings which run through its whole length, from the baſe quite to the top; in an hour after it ſpread in breadth, the red colour turned by degrees to white, this

this alteration having begun at the point, continued ſucceſſivelyto the baſe,ſome minutes after this alteration the column was diſſipated.

This *phænomenon* is the firſt of all thoſe that have appeared this year in different parts of *Europe*. It was obſerved *March* 15, two days before thoſe which were ſeen in *Germany*, *England*, and *France*, and almoſt a month before that which we have obſerved. It ſeems to have ſome conformity with thoſe pyramids of light, which were obſerved in *England*, *March* 17, in different parts of the heavens, and ſeparated from the horizontal light.

Among the memoirs of the royal ſociety of ſciences at *Berlin*, we find the obſervation of three ſuch *phænomena* made at *Copenhagen* in *February* and *March* 1707. I believe theſe obſervations are M. *Roemer*'s, for the two letters O. R. which are in the title mean, if I am not miſtaken,*Olai Romer deſcriptio*, which is the name of that celebrated mathematician. In the firſt of theſe obſervations, which is *February* 1, they ſaw about 11 at night a ſort of arch which extended from the W. N. W. to the N. N. E. and which afterwards in the moſt diſtant part from the horizon was 3° in height. This arch afterwards roſe, and became brighter through its whole extent. About ½ an hour after midnight there was formed by degrees a new arch above the firſt, and at 1 they ſaw rays come out of it like beams, which roſe up as if they had been darted: theſe rays began to appear firſt in the upper arch, then in the lower, and roſe 4° above the upper arch; at two this *phænomenon* was in its full brightneſs, it was very much elevated above the horizon, and extended by degrees over the whole heaven. A fog, which roſe afterwards made this *phænomenon* diſappear.

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The same astronomer relates the observation of a like *phænomenon*, but not so fine nor so perfect as the first. It appeared on the first of *March* the same year, from 10 at night till 1 in the morning in the same region of the heavens.

In the last place he speaks of a third, which appeared on the 6th of *March* the same year, between 7 and 8 at night, which he says was less regular than the first. It had this particularity, that most of the rays, which came out of this arch, arrived quite to the upper part of the heavens, which were soon after cloudy and the *phænomenon* disappeared.

He observes that this *phænomenon* appeared brighter and larger at *Pinembourgh*, 2 leagues from *Copenhagen*; whence he concludes it was vertical, and consequently low and near the surface of the earth.

There is also in these memoirs another observation of the same *phænomenon*, made the same day, *March* 6, 1707, at *Berlin* by M. *Kirchius*. This astronomer observed it at 8 in the evening, in form of a rainbow, but broader, and its length possess'd about 100° at the horizon. The upper part of this arch was elevated 8 or 10° above the horizon, whence proceeded luminous rays, which were directed toward the zenith. He afterwards saw a second arch appear above the first, at the height of 30°, but was neither well terminated nor well continued.

If the observations of this *phænomenon* made the same night at *Copenhagen* and at *Berlin* were a little better circumstanced, one might have been able to determine the distance of this meteor above the surface of the earth; but we are in doubt whether the upper arch seen at *Berlin* is that which was seen at *Copenhagen*, or whether it is not rather the lower

lower one, which ſeems more probable; for at *Copenhagen* this *phœnomenon* was not viſible becauſe of the clouds till between 7 and 8, and at *Berlin* the lower arch was obſerved at 8, and the upper one ſeems not to have been ſeen till nine, when the *phœnomenon* had been for above an hour more viſible at *Copenhagen*. If we ſuppoſe it to be the lower arch that was viſible in both places, which is the moſt probable, and that the difference of the height of the pole between *Copenhagen* and *Berlin*, which are almoſt under the ſame meridian, is 3°, we calculate the diſtance of the *phœnomenon* above the ſurface of the earth 15 leagues of *Paris*. If we ſuppoſe that the upper arch ſeen at *Berlin*, is that which was ſeen at *Pinemberg*, or *Copenhagen*, the diſtance of the *phœnomenon* from the ſurface of the earth will be 3 times as great.

The obſerver of *Copenhagen* ſhews by his diſcourſe, that he had obſerved other like *phœnomena* the years before, and adds, that they are very common every year in *Norway* and *Iceland*. *M. L.* relates that a *Saxon* hiſtoriographer mentions a great northern light, ſeen for an hour, on the night of *St. Stephen* the martyr, in the year 993.

We have another obſervation of a northern light made at the beginning of the laſt century, by M. *Gaſſendi*, and mentioned in different parts of his works.

September 12, 1621. Being near *Aix* in *Provence*, when the *crepuſculum* was almoſt ended, he ſaw above the horizon to the northward a light, which appeared like a very bright day-break, and being contained between the ſummer riſing and ſetting occupied 60° of the horizon. The upper extremity was formed in an arch, which roſe inſenſibly to the height of 40°. The matter, which formed

formed this appearance, was ſo thin, that it did not obſcure the ſtars over which it paſſed. It was traverſed from the horizon to its upper part by ſeveral luminous beams, which made its extremity unequal. Theſe beams alternately bright and dark were each of them 2°, and perpendicular to the horizon.

This *phænomenon* appeared not only in *Provence* and thereabouts, but M. *Gaſſendi* learned that it had been ſeen at *Cioutat*, *Digne*, *Grenoble*, *Dijon*, *Paris*, *Rouen*, *Toulouſe*, *Bourdeaux:* and in the camp before *Montauban*, which was then beſieged.

He affirms alſo, that beſides this *phænomenon*, there were 4 others like it, one in *Februa*:*y*, one in *April*, and two in *September*, but that they were not ſo fine as the firſt, and that all theſe appearances had been followed by ſome days of fair and calm weather.

By the obſervations juſt related, it appears, that all theſe *phænomena* are almoſt of the ſame nature, though ſome have been greater, brighter and better terminated than others; that ſome ſeaſons are better ſuited to theſe appearances, which are *February*, *March*, *April*, and *September*, though according to the teſtimony of the *Saxon* annaliſt, it appeared alſo in *December*, near the winter ſolſtice; that theſe *phænomena* have appeared in fair weather, and after one or more hot days. This is teſtified by *Gaſſendi* and *Kirchius*, and it happened alſo in the two appearances of this year.

VI. *Obſervations on the matter which colours the falſe pearls, and ſome other animal ſubſtances of the like colour; on occaſion of which it is attempted to explain the formation of the ſcales of fiſhes; by M.* de Reaumur; *tranſlated by Mr.* Chambers*.

The art of making falſe pearls, ſuch as are now wore, is but of a late ſtanding, being unknown a little above ſixty years ago, though much ſought after for many ages before. The value put upon true pearls had occaſioned infinite attempts to counterfeit them; and few authors of ſecrets, that is perſons who give the ſuggeſtions of their own imaginations for ſure and approved methods, but have left receipts for the compoſition of pearls; at length however we are arrived at a perfect imitation, ſuch as that the eye, which ſeems the only proper judge of this kind of beauty, can hardly diſtinguiſh between the natural pearls and the fictitious ones, or if it do diſcover theſe latter, it is only by their want of defects enough. I have known a ſtring of falſe pearls which the moſt expert jeweller would value at an immenſe ſum if he had ſeen it on the neck of a princeſs; and hence the true pearls have ſuffered conſiderably in their price.

Though it be a reproach on our nation that we are better at improving than diſcovering, yet both the invention and perfection of this little art is our due, which at preſent employs a great number of workmen in *Paris*.

The chief ingredient in the compoſition of theſe pearls, which gives them that beautiful ſilver colour

* November 14, 1716.

lour, is called by the workmen oriental eſſence, being the produce of a little fiſh common enough in the *Seine* and ſome other rivers, under the name of *Able* or *Ablete*, in *Latin Albula*; in the *Sein* it is rarely above four inches long, and in moſt reſpects reſembles a ſmelt, except that its ſcales are of a brighter ſilver hue, theſe ſcales being ſcraped off as uſual, are put in a baſon of fair water, where they are briſkly rubbed or ground together, the matter ſeparating from them gives the water a ſilver tincture, upon which pouring off this firſt water into a large glaſs, they caſt new water upon the ſcales, then grind them a-new, and pour off this water into a ſecond glaſs when it has acquired its colour, which operation they repeat until the water will tinge no more. That poured into the glaſs being left to ſettle for ten or twelve hours, the ſilvered matter precipitates to the bottom, as being the heavieſt, and leaves the water a-top perfectly clear, this they decant ſo as to leave nothing in the glaſs but a thickiſh liquor of the conſiſtence of oyl, and the colour like that of pearls; and this is what they call oriental eſſence.

To make uſe of this liquor, all that remains is to mix it up with a little fiſh-glue.

It was at firſt only made uſe of externally, to waſh or verniſh beads made of wax, alabaſter, or glaſs, which were hereby rendered perfectly like pearls, but they laboured under this defect, that the waſh not not being proof againſt moiſture, the ladies could not wear their necklaces in hot weather without being in danger of ſtaining their ſkin; but an eaſy remedy was found for this defect by blowing hollow thin glaſs beads of a bluiſh colour, and dropping of the oriental eſſence in each of them which is the method in uſe, the workman blowing the eſſence in with a pipe, and taking the bead

 between

between two fingers ſhakes it a few moments, by which means the liquor is ſpread over the whole inner ſurface of the bead, ſo that the oriental eſſence is only ſeen through the glaſs, as the tin and quickſilver are only ſeen through looking glaſſes. The glaſs being extreamly thin, diminiſhes ſcarce any thing of the luſtre of the eſſence, but its bluiſh colour even gives the falſe pearl a nearer reſemblance to the true ones. This done the pearl is laid on a baſket among many more, where they are ſhook together for ſome hours until they be dry. Laſtly, to make them heavier and ſtronger they fill them with wax.

Having ſhewn the uſe which art makes of this oriental eſſence, let us now examine its nature, and how it is lodged in the ſcales; to ſpeak hereof with the more exactneſs we ſhall begin with ſetting aſide its name eſſence. Of itſelf it is no more a fluid than a fine ſand, or than powdered talc mixed with water; but it is not eaſy to draw it from the ſcales without waſhing; and to be uſed it requires like many painting earths to be mixed up with water. Upon viewing it with a microſcope, or large magnifier, it is eaſy to diſtinguiſh it from the fluid it floats among, and to perceive that itſelf is not fluid, but in lieu thereof a cluſter of little corpuſcles of a very regular figure; in effect they are ſo many *lamellæ* or little plates, the greateſt part whereof are cut perfectly ſo as to form rectangles about 4 times as long as broad, though ſome of them have their extremities rounded, and others terminate in points, but they are all extremely thin, to ſuch a degree that one can perceive no thickneſs they have; whence it follows, that the matter they conſiſt of muſt be very firm and ſolid, ſince notwithſtanding the vigorous frictions made uſe of to ſeparate them from their ſcales they

they are neither broke nor bent, at leaſt we find no broken nor bent ones by the miſcroſcope, but all appear nearly of the ſame ſize, and terminated in right lines on their large ſides.

It is eaſy to conceive what a luſtre they exhibit with the help of a microſcope, to which the moſt burniſhed ſilver has nothing to compare; and it is obvious likewiſe their being ſo thin and ſhaped ſo regularly, they are extremely proper to range themſelves on glaſs, and appear thereon with the luſtre and poliſh of pearls. They ſeem in a continual agitation till they be precipitated to the bottom of the water, and give way ſo ſpeedily to the ſlighteſt motion therof, that I do not doubt but they would have been taken for inſects, by thoſe who are ready to give that name to whatever is continually moving in fluids.

We have already obſerved that it is by waſhing the ſcales this matter is procured, to which it muſt be now added, that there is none unleſs by accident on their external ſurface, and that the ſurface contiguous to the body of the fiſh is always covered over with them; hence pulling of a ſcale, and rubbing the finger over its outer ſurface, it takes not the leaſt colour; whereas paſſing it over the other ſurface it is viſibly ſilvered.

The diſcovery made by a modern anatomiſt of an unctuous liquor which ſmeers the outſides of fiſhes, might make one ſuſpect that this matter may ſerve for the ſame purpoſe, but there is a deal of difference between them, beſides that the unctuous matter is of the colour and conſiſtance of a real gelly, and furniſhed by very conſiderable veſſels, as I have likewiſe obſerved in the *torpedo*. Both thoſe matters are found in the ſame fiſhes, and they are eaſily diſtinguiſhed from each other both by their qualities and uſes.

Nor

Nor muſt it be ſuppoſed, as I at firſt was inclined to do, that the matter of this oriental eſſence is that exhaling from the bodies of fiſhes by way of inſenſible perſpiration, and that it ſticks and gathers upon the ſcales as on a kind of roof or vault, for we find it ranged with too much order and apparatus to have come in this way, being covered over with membranes and even lodged in veſſels; and hence if with a pin's point we endeavour to raiſe any part from the ſhell, all the reſt, or at leaſt a good part, uſually comes along with it, by reaſon of the common membrane it is contained in. This membrane being examined by a microſcope, diſcovers, beſides the blood veſſels ſpread over it, a great number of thickiſh fibres all parallel to each other, and perpendicular to the length of the ſcale; taking the length of the ſcale the ſame way, as that of the fiſh. Theſe which we here call fibres, are really veſſels or tubes wherein our ſilvered matter is contained, whence upon ſqueezing ſome of theſe tubes about their middle, I have frequently ſeen cluſters of theſe *lamellæ* at their ends, ranged over each other like a pack of cards half gathered together, the length of each *lamella* being in the ſame direction as that of the tube.

As the ſcale is not equally broad every where, theſe veſſels are not of an equal length, thoſe towards the extremities being ſhorter than thoſe in the middle; we can ſometimes ſee both their ends one of which terminates in one edge of the ſcale, and the other in another; thoſe however between the two extremities of the ſcale are not near ſo long as the ſcale is broad, but it is the extremities of different tubes that are perceived in different places; nor is there any room to ſuſpect, that we here take the places where the tubes have been broke, for thoſe where they naturally terminate; their

heir natural figurewould eaſilydiſtinguiſhwhat had been made by ſuch rupture. They are all cylindrical the greateſt part of their length, and terminate in a point of their extremities.

Let us now ſee whether what has been ſhewn of the nature of this ſilvered matter and the veſſels it is contained in, may not enable us to determine its uſe. By conſidering what has been obſerved, one can ſcarce forbear imagining that the ſcales of the fiſh owe their formation and growth to this matter, the hardneſs and tranſparency of the *lamellæ* ſhew that their nature is very different from that of the fleſh of the animal; and near a-kin to that of the ſcales. In fine the bare figure of the ſilvered *lamellæ* ſeems to ſuggeſt they are ſhaped like ſo many little bricks, in the moſt proper manner for building the ſcale. The veſſels they are contained in compleat the proof; the apertures thereof being ſo diſpoſed as to furniſh *lamellæ*, not only for enlarging the whole ſurface of the ſhell, but multitudes of them terminated in all the other parts thereof; ſo that if we ſay with *Lewenhoeck* that each ſcale conſiſt of an infinity of ſcales, laid one over an another, or more ſimply, of an infinity of *ſtrata* whereof thoſe next the body of the fiſh are the largeſt, there will be veſſels every where to furniſh matter to form them.

Theſe *ſtrata* ſhew a beautiful kind of workmanſhip upon the ſcales. Viewing them with a microſcope we find them covered with wonderful art, and diſcover a prodigious number of concentric flutings thereon, too fine as well as too near each other to be eaſily told; but the laſt keeps pace with the circumference of the ſcale, and the reſt have the like curvity; they are formed by the edges of each *ſtratum*, and the limits whereof they denote, and at the ſame time denote the different ſtages of

the

the growth of the ſcale, as we have elſewhere ſhown; that the like flutings ſhew the periods of growth in ſhells. It follows hence that theſe flutes ſhould be upon the ſcales, but the ſcales are ſo thin and tranſparent that it would be difficult determining on which ſide they are fluted, were one only to obſerve them near their edges. But about the middle where the ſcale is thickeſt, we ſee none or ſcarce any of theſe flutings underneath, though they be very apparent above. Laſtly, if obſerving the upper ſide of the ſcale in two ſeveral poſitions, putting in the firſt the edge we view nearer the eye than the middle of the ſcale, and in the ſecond the middle nearer the eye than the edge will look at the diſpoſition of the flutings, with reſpect to each other is eaſily found, for in the former poſition they all appear raiſed one above another, and a larger part of each is ſeen, the caſe being now the ſame as when we view the ſteps of a ſtair-caſe from below upwards; whereas in the other poſitions, the flutings are placed like the ſteps of a ſtair-caſe viewed from above downwards.

The flutings above mentioned are traverſed by others, proceeding from the centre of the ſcale. What I call the centre is not preciſely the middle, but a point incompaſſed by the flutings parallel to the circumference of the ſcale. Thoſe which proceed from the centre are much bigger than the others, and the number is fixed being ten in the *Able*; ſix whereof are diſpoſed like the ſticks of a fan, and terminate in that part of the ſcale neareſt the tail; the other four being diſpoſed like the former and terminated in the other part of the ſcale next the head. The ſtrait flutings are concave, whereas the crooked ones are in relievo or convex, and ſeem intended to lodge blood veſſels.

There

There are ſome ſcales, and even in the *Able*, which have more work in them ſtill. On this fiſh we ſee two, as it were, dotted lines proceeding each from that part of the gill neareſt the back, and each forming a cavity which looks towards the belly of the fiſh, terminates in the middle of the tail; now the ſcales which theſe two lines paſs over, have beyond any of the reſt a very extraordinary kind of little tube, on their external ſurface. Theſe tubes placed endway of each other deſcribe the two dotted lines abovementioned. Each tube is a little raiſed above the ſcale, and has the figure of a truncated cone, the diameter of one of its ends being larger than that of the other; the thick end is neareſt the head, and commences where the ſcale ceaſes to be covered by the preceeding one, its direction being pretty much the ſame as that of the fiſh. Theſe little tubes fitted end to end form a continued canal, which probably ſerves for the conveyance of ſome matter analogous to the unctuous matter abovementioned, which ſmeers the body of ſeveral fiſhes.

Nor is it only in the ſcales that the ſilvered matter is found, but there are two other large funds of it in the body where it may perhaps be prepared. When the *Able* is ſcaled we ſtill find it ſhining as before, the reaſon is that immediately under the ſkin, upon which the ſcales are placed, there is a membrane like that which covers the ſcales themſelves, and filled like it with ſilvered *lamellæ*, being in all likelihood the ſource, from whence they are ſupplied to the tubes of the ſcales; but by what canal they are conveyed thither I have not yet diſcovered, perhaps it may be for the receiving and diſtributing of theſe, that the five or ſix apertures obſervable in the membrane which covers the ſcales

are intended. These apertures are ſhaped like funnels, and conſiſt of three or four ſtrings placed one againſt another.

The other fund of ſilvered matter, is in the cavity of the fiſhes belly, and renders the membrane which incloſes the ſtomach and inteſtines all ſhineing. Upon examining the *lamellæ* contained herein, with a microſcope, I found them of the ſame figure as thoſe of the ſcales; only they ſeemed to be ſmaller; though I dare not aſſert ſo much, for fear I ſhould have imagined I ſaw, what I ſought to ſee.

Yet I have one remark which ſeems to prove that the ſilvered leaves are not ſo ſolid and conſiſtent in the cavity of the belly as under the ſcales; for endeavouring to gather ſome of the ſilvered matter by rubbing the membrane between my fingers in water, I found that a friction, ſuch as that employed upon the membrane of the ſcales, or even a more vehement one, would take up nothing from this membrane, though it be not ſenſibly thicker than the other. It would fold between my fingers and become a roll, or even a ball, but would yield nothing, without taking another method to tear it off: does not this ſeem an indication that the *lamellæ* contained in this membrane have not yet arrived at ſo much hardneſs as elſewhere; but that being ſoft they give way, and fold with the membrane they are in? whereas in other places having a conſiſtance like that of ſcales, they pierce the tubes which incloſe them; upon ſqueezing them againſt their ſides. In a word the one only contains the rudiments, or as it were embrio's of the *lamellæ*, and the other thoſe arrived at their ſtate of perfection.

That univerſality obſervable in the laws of nature, requires that the ſcales of all fiſhes ſhould be formed

formed after the ſame manner, and conſequently they muſt all have a matter compoſed of a multitde of little hard *lamellæ*, like thoſe obſerved in the *Ables*, if thoſe *lamellæ* have really the uſe we have attributed to them. Accordingly I have ſought for them in ſeveral ſpecies of fiſhes, and have had the fortune to find them; and what is more have always found them of the ſame figure, and always thin and of a bright ſilver colour, even in fiſhes of another colour, as in carps, which appear the moſt gilded.

An experiment, which the pearl-makers make oftener than they deſire, may ſeem an objection againſt what we have ſaid of the ſcaly nature of the ſilvered *lamellæ*. If the oriental eſſence be kept ſeveral days eſpecially in ſummer time it putrifies and acquires a very fetid odour, like that of a rotten fiſh. At the ſame time its colour alters, and turns yellowiſh, until at length the whole ſilvering diſappear, in ſtormy weather theſe changes will happen from one hour to another. Now the ſcales are not of ſo corruptible a nature, but are proof both againſt the moiſture and heat of the air. ——But it muſt not be here ſuppoſed that the *lamellæ* are altered when the eſſence is, for this latter not only conſiſts of *lamellæ* but of abundance of fleſhy particles belonging to the tubes and membranes they were lodged in, it being impoſſible by lotion to procure them perfectly free of all mixture. If now the eſſence be uſed while theſe fleſhy matters are yet freſh, they will dry on the bead, and in this caſe no more change their colour than the fiſh-glue mixed with them. Thus every body knows that a fiſh well dried will keep many ages, but if the fleſhy matters corrupt in the water they looſe their whiteneſs, turn glutinous, and ſtick to each other and to the *lamellæ*, which otherwiſe

muſt ſoften in the water, without corrupting as the ſcales of fiſhes do, which are much thicker, and form clots without either luſtre or tranſparency, ſo that nothing elſe but ſuch little clots is ſeen at the bottom of the glaſs where the eſſence was, the water over them being quite clear and ſcarce having any thing ſilvered in it.

I have tried whether there might not be a means of preſerving the eſſence by boiling it, but inſtead thereof the whole turned into opaque clots like eſſence which had been putrified of a long time, which indeed is no wonder for boiling water quickly renders fleſhy matters glutinous, and makes of them a kind of opaque fiſh-glue, which entangles the ſilvered parts.

Spirit of wine ſeems a ſurer means of preſerving the eſſence, as it prevents fleſh from putrifying, but pouring ſome upon my eſſence it altered its colour a little and turned more whitiſh and a little leſs tranſparent. Accordingly it muſt have cockled the fleſhy particles, but there were no clots formed, and the *lamellæ* ſtill remained ſeparate; ſo that upon viewing them with a microſcope, I found them with their firſt luſtre, even after many months. But pouring a large quantity of water in eſſence thus preſerved by means of ſpirit of wine, in a few days time I found clots formed, the water having ſoftened the fleſhy particles and rendered them glutinous. To the naked eye theſe clots or grumes appear opaque, though by the microſcope they are only found a cluſter of *lamellæ*, with their natural figure and almoſt all their former luſtre.

This may ſerve to ſolve a difficulty ariſing hence; that the ſcales do not ſhine tho' formed of a matter which does, ſince the grumes do not ſhine though only cluſters of *lamellæ*. It is true there may be ſome

ſome fleſhy particles with them; but it is no leſs probable, that there is ſome viſcid matter in the compoſition of the ſcales which faſtens the *lamellæ* one to another. Be this as it will, it is enough that the ſurfaces of the ſcales be uneven to hinder the ſhining of the *lamellæ* from being perceived therein.

As to the ſcales being more tranſparent than the oriental eſſence, there is no difficulty in accounting for it; nor is it any more a wonder that the colour of the ſcale ſhould differ a little from that of the eſſence. A few parallel facts will ſhew the cauſe thereof. A white diamond is the moſt tranſparent of all bodies, and yet the powder procured from it by grinding is opaque, and which is more browniſh. *Dutch* ſugar-candy is yellowiſh, and yet when powdered yields a very white powder; and among black amels uſed by enamelers ſome when ground yield a blue powder, and others a bright coffee coloured one.

Nothing can equal the vivacity of colours of ſome fiſhes when juſt caught, which they likewiſe owe to our ſilvered matter, and the ſame matter likewiſe ſerves to diverſify their colours, being an ingredient in their compoſition; thus what would only appear for inſtance of a reddiſh yellow, by the help of this matter becomes gold coloured; this I have frequently obſerved in the *Able*, the bottom of the large ſcales covering the gills whereof, is lined like the reſt with a ſilvered membrane, and conſequently appears quite white, but when upon a veſſel's burſting the blood extravaſates under this membrane, all the places the blood ſpread to becomes of a gold colour. Every body knows that our gilt leather hangings are only ſilvered over, and a reddiſh varniſh applied thereon, the ſilver viewed through ſuch varniſh appearing like

gold;

gold; and the ſame thing is here found though in a contrary order, the blood colour viewed through our ſilvered matter appearing of a gold colour. Thus if in ſome ſpecies of fiſhes a thin tranſparent layer of our ſilvered matter be ſpread upon *Plexus*'s of blood veſſels, whoſe *parietes* are very thin, the ſcales will appear gilded. If the *parietes* of ſuch veſſels be thick, the veſſels will then be of a blue colour like our veins, and the fiſh appear of a ſhining blue, and it is not impoſſible but a mixture of veſſels proper to exhibit a yellow or gold colour, with other veſſels diſpoſed to exhibit blue, may give ſome fiſhes a green colour; at leaſt it is certain the vivacity of theſe colours ariſes from our ſilvered matter; though the generality of them are but of a ſhort duration, after the fiſh is taken out of water the veſſels drying ſhrivel, and looſe their tranſparency; ſo that the ſilvered matter can no longer have its effect.

We have ſeen the uſe which both art and nature make of the ſilver'd matter of fiſhes; ſo that all now remaining, is to ſpeak of ſome other animal matters analogous thereto. We begin with that of an inſect harbouring in books rarely look'd into, which reſembles the *Able* by its ſilver'd colour, and has ſomething likewiſe of its ſhape, ſetting aſide the legs. *Hook* gives us a figure of it in his *Micrographia*. Upon the leaſt touching it with the fingers, it becomes ſhining, as if it had been rubb'd with oriental eſſence. This little inſect is cover'd all over with ſcales even to its very feet; but they are ſo little faſten'd, that the ſlighteſt friction pulls them off; and 'tis theſe ſcales that tinge the fingers. When viewed by a magnifier they appear of a regular figure, like that of the ſcales of fiſhes; and 'tis highly probable they derive their luſtre from a like ſilver'd matter

matter; but there is no poſſibility of perceiving the *lamellæ* in theſe ſcales, when themſelves are almoſt as ſmall as the *lamellæ* which ſilver the ſcales of fiſhes.

There are ſome ſpecies of butterflies likewiſe which cannot be touched by the fingers without leaving a ſilver'd duſt thereon; and in the general all thoſe beautiful colours ſo wonderfully diverſified in the wings of moſt of theſe inſects conſiſt in duſts which are eaſily taken off. Since the uſe of microſcopes has been familiar, 'tis well known that theſe duſts have regular and very remarkable figures, which are different in different ſpecies of butterflies, and even in different parts of the ſame wing. Thoſe which colour the wings of the moſt common butterflies, reſemble a flower ſuſtained by a ſhort pedicle, which flower is ſometimes like a tulip with only three leaves, ſometimes with four leaves, and ſometimes with five. In the butterflies call'd *Pan*; they reſemble a leaf of a plant, or rather a fan with four or five deep and broad indentures; the *farinæ* which border the wings are much longer, forming a kind of cones, whoſe baſe divides into two other conical branches, the *vertex* of the chief cone is inſerted within the membrane of the wing, 'tis theſe long *farinæ* which form the fringes wherewith their wings are bordered.

Theſe kinds of *farinæ* are called feathers by ſome authors, tho' the name ſcales ſeems to ſuit them better. Not that there is any great difference in nature between horns, ſcales, and feathers. But barbs or beards have always hitherto entered the character of feathers, and our *farinæ* have none. Some butterflies, 'tis true, have feathers; and I have obſerved one ſpecies in particular, whoſe wings are each of them compoſed of five feathers,

feathers, perfectly ſimilar in figure to thoſe of birds. But theſe feathers are different from the *farinæ* which cover the wings, and conſequently theſe latter ſhould be diſtinguiſhed by a peculiar name.

Accordingly the uſe of the *farinæ* is the ſame with that of the ſcales of fiſhes, but has no relation to that of feathers, their only office being to cover the wings; they are ranged thereon after the ſame manner as ſcales on the body of a fiſh, that is ſomewhat like tiles on the roof of a houſe, with this difference, that each grain or *farinæ* has a pedicle inſerted into the membrane which forms the wing. They are diſpoſed in rows, and thoſe of one row cover part of thoſe of the following one; upon taking them off the wing, we find ſeries's of little holes darker than the reſt, laid out from ſpace to ſpace in even lines, into which the pedicles of the *farinæ* had been implanted. This is a deal of work for the meer wing of a butterfly; but upon a little obſervation of the works of nature, one ceaſes to be ſurprized at ſeeing infinite pains beſtowed upon the ſlendereſt ſubjects.

VII. *A ſolution of ſome difficulties relating to the formation and growth of ſhells; by M.* de Reaumur*. *Tranſlated by Mr.* Chambers.

I have explain'd, in a preceding memoir, the formation and growth of ſhells, and at leaſt flatter myſelf I have proved therein that they grow like ſtones, by a mere appoſition of matter, or, to ſpeak with the ſchools, by *juxtapoſition*, and not as plants and animals grow, by intus-

* Dec. 19, 1716.

intus-susception; and the experiments whereon my opinion is founded, are here laid down at large. Since which *M. Mery* having considered river muscles, has taken occasion to examine their shells, and maintains on the contrary, that they can only grow by *intus-susception*; the reasons whereof he delivers in a memoir full of curious observations upon muscles, having had the candor withal not to mention that his reasons were opposite to what I had advanced on the growth of shells, and at least subtracted from my system of all the *bivalvæ* or two-leav'd shells.

The proofs, or rather, as M. *de Fontenelle* calls them, the difficulties he alledged, seem to deserve a solution; but as it was only on experiments I had determined myself, I was resolved only to answer by experiments, in order to cut off all matter for further reply. Accordingly I inclosed muscles in vessels, which I put in the river *Marne*, in a kind of covered boats call'd *Boutiques*, or fish-pools; but several accidents befalling my muscles for several years together, rendered my endeavours ineffectual, and oblig'd me to defer the intended solutions. However, as the experiments in question seem only necessary for a redundancy of reasons, and that a too long silence might have been taken for a tacit acquiescence, I have now thought proper to give an answer to those difficulties.

I lay down as my principle, that a snail's shell grows by meer *juxta-position*; that the animal inlarges it in proportion as it grows itself, and this by reason that part of his body which emerges beyond the old shell, emits a stony juice, which hardening forms a new piece of shell very thin indeed at first, but which soon becomes thicker by the apposition of new *strata* under it, and

and thus does the growth of this animal's ſhell proceed. This I ſay I lay down as a principle, eſteeming I have a right to do ſo upon the proofs laid down in my former paper, which I take to be as demonſtrative as a phyſical proof can be.

Now this principle is laid down, let us examine the reaſons which have induced M. *Mery* to ſuppoſe muſcles ſhells grow in another manner. Theſe are reducible to two, the firſt and ſtrongeſt is that the muſcle is faſtened to the internal *parietes* of its ſhell by 8 muſcles, or rather that it is faſtened in 4 places to each of the two pieces or leaves of the ſhell. Now if the ſhell grew by *juxta-poſition*, *it would follow*, ſays he, *that the muſcles muſt looſen themſelves therefrom, and be continually receding from the place of their firſt faſtening, as often as a new leaf or* ſtratum *is formed*; *a* phœnomenon, adds this learned anatomiſt, *which I have ſeen nothing of in any of the muſcles I have hitherto diſſected in all ſeaſons.*

Now beſides that, there is no inſtance of ſuch a ſhifting of place either in animals whoſe muſcles are faſtened to bones, nor even in thoſe which have no bones, as crabs, lobſters, craw-fiſhes, &c. *whoſe body is only inveſted with cruſts or ſcales which ſerves them in lieu of bones, and in which all the muſcles have their origin and inſertion. Is it not much more probable that all the* ſtrata *of a muſcle's ſhell are formed at the ſame time, like the ſhells of thoſe fiſhes, than after one another?*

'Tis certain the ſhells of muſcles cannot grow by *juxta-poſition*, or as thoſe of ſnails do, unleſs the muſcles which faſten this fiſh to its ſhell ſhift their hold, and 'tis likewiſe true that ſuch ſhifting of large muſcles or ligaments, is very difficult to explain or even to imagine; but then it is not certain that there are no inſtances of ſuch a ſhifting.

ing. The difficulty at bottom is the ſame to make one ligament ſhift as to make 4. Now there is one which certainly ſhifts in the ſnail, as will neceſſarily follow from the growth of its ſhell by appoſition. Every ſnail is faſtened to its ſhell by a muſcular ligament, and when the large garden ſnail is born, its ſhell only makes a little above one circumvolution of a ſpiral, whereas it makes above 4 when arrived at its laſt term of growth. Now in a ſnail whoſe ſhell contains 4 circumvolutions, the muſcle is faſtened upon the navel of the ſhell between the ſecond and third circumvolution, ſo that in whatever part you ſuppoſe it to have been faſtened in the ſhell of the new born ſnail, 'tis certain it muſt have travelled almoſt two circumvolutions of the ſpiral. All the difficulty therefore is to make the ligaments of the muſcle, travel like thoſe of the ſnail, and whatever means nature has employed for that end in ſnails, who doubts but ſhe may have employed it in like manner for muſcles. M. *Mery* adds indeed that he has diſſected muſcles in all ſeaſons, and yet never ſaw this *phænomenon*. But this adds no great ſtrength to his proof. M. *du Verney* has alſo diſſected ſnails in all ſeaſons, and I have had occaſion to diſſect ſeveral myſelf; yet I believe neither of us have ſeen how this ligament of the ſnail ſhifts, yet it does ſhift neverthelеſs.

We have therefore an inſtance of the ſhifting of muſcles, and in our own caſe too, that is in that of the growth of ſhells. M. *Mery* need not have added that we ſee no inſtance of ſuch ſhifting, either in animals whoſe muſcles are faſtened to bones, nor in *crabs, lobſters, and crawfiſhes, which have only cruſts or ſcales in lieu of bones*. All theſe negative facts would prove nothing againſt us; the inſtance of crabs and lobſters was

Cc 2 very

very unhappily cited, it being precifely that I fhould have pitched upon to prove that the mufcles do fhift in fome animals. Thefe, as is well obferved by M. *Mery*, are covered with crufts or fcales, which ferve them in lieu of bones, and in which all their mufcles have their origin and infertion. Now I have fhewn in a memoir printed fince that of M. *Mery*, that thefe animals diveft themfelves every year of their coats and fcales, and of all in general that is hard and fcaly, and might ferve for bones about them, fo that we have here no lefs than a general fhifting of all the mufcles, at leaft once a year, fince they are every year faftened to a different fhell from that of the preceeding year.

Let nobody urge that this is not a fhifting, by reafon the mufcle does not change place on the fame fhell, as in the cafe in queftion: What makes the real fhifting or difplacing of a mufcle, is its being faftened to another part than that it was faftened to before.

'Tis true notwithftanding this kind of fhifting, that the mufcles of lobfters can never be faid to be without a point of fupport, or points of infertion, by reafon a new fhell is formed under the old one, between it and the mufcle, to which while it is forming the mufcle faftens itfelf; and 'tis probable this new fhell is not formed all at once under each mufcle, that the mufcle may always have fomething to faften to.

Neither muft we imagine that there is a time when all the mufcles of the mufcle-fifh are loofe at once, fo as to leave the animal at large in its fhell, tho' in that cafe it would only be in the condition of a lobfter which has juft put off its old fhell. It is more natural to fuppofe, that there is no time

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when each muſcle is not partly at leaſt faſtened to the ſhell.

M. *Mery* obſerves, that each ſhell is lined with a thin membrane, into which the extremities of the muſcles or ligaments are implanted. This membrane when it grows muſt extend towards the edges of the ſhell which is the only ſide it can extend on; ſo in the ſpiral ſhells we find that the membranes only extend on the ſide of the aperture. As this membrane advances the ligaments advance with it, and as their growth proceeds inſenſibly, and perhaps may not always correſpond to the ſame times; there is no time when this membrane and the ligaments are entirely aſunder: ſuppoſe for inſtance, that this membrane grows in a kind of wavy direction, I mean in different bands, or *faſciæ*, and that the *faſcia*, for inſtance, neareſt the edge of the ſhell grows, and after this another, and ſo more ſucceſſively, until under the ligaments; and if we ſuppoſe theſe very narrow, though in extending they ſhould come to looſen from the ſhell, there would be no time when the ligaments had not faſtened, nor is it even impoſſible, but that the *faſcia* might grow though ingaged in the ſhell. M. *Mery* may ſay that we have no inſtance of any growth in this kind of undulation, but before his memoir, had we an inſtance of an animal without either veins or arteries; which had no fluid in its heart, but the water taken in by its mouth, where there is no circulation but in the heart and auricles; an animal which propagates without copulation, and whoſe ſeed is only caſt upon the eggs after they are laid? all of them facts at leaſt as difficult to beleive as the preceeding conjecture. However I only regard it as a conjecture, and give it not ſo much to ſhew how this ſhifting is

is effected, as to ſhew that there are ways wherein it may be effected.

We come now to M. *Mery*'s ſecond difficulty whoſe ſolution will be more eaſy than that of the former. *Muſcle ſhells*, he obſerves, *are viſibly compoſed of ſeveral leaves, or* ſtrata, *laid one over another: and which by reaching beyond each other make diſtinct bands on their external ſurface.* Which ſeems to prove that theſe leaves are formed one after another, and repreſent the ſeveral ſtages of growth, but what makes the difficulty is that M. *Mery* has found as many bands on the little ſhells, as on the largeſt, and that theſe bands inlarge, as the body of the muſcle grows.

I will not ſay it is a random objection to affirm that there as many bands on the little as the large ſhells, and yet this ſeems to be the caſe; the numbers both of the one and the other being hardly poſſible to be told. I ſhall however agree that ſo far as one may judge, by the firſt caſt of the eye, there often appear as many *ſtrata* on the ſmalleſt ſhells as on the largeſt, and ſhall even add which perhaps will be too much for M. *Mery*, that when the ſhells are very old, there appear more on the ſmalleſt than on the largeſt; the reaſon is that ſuch thin claws are not proof againſt the friction of the ſand nor even of the water, ſo that it would be in vain to ſeek for them in the old ſhells, what ſome of them had beyond others having been carried off. Upon other ſhells there grows a greeniſh mouldineſs, or a kind of moſs, which ſpreads like a membrane over the ſhell, and hides multitudes of the ſmaller bands. In fine, it is certain that among the bands which mark the different ſtrata, there are ſome of them larger in the larger ſhells than in the leſs, but this not be-

cauſe

caufe the little bands have been inlarged, but becaufe when the fifh is arrived at a certain bulk, its growth becomes quicker, fo that as the bands are what it has grown in a certain time, the bands now formed become larger than thofe before, and accordingly the largeft are ufually found near the circumference of the fhell, unlefs in fome other places the friction happened to have made one out of feveral of the fhells. Garden fnails afford a good exemplification of this, the feveral ftages of growth are here well denoted; when they are little there are frequently feveral of thefe terms in a line's compafs, whereas when large there are fometimes feveral lines between one term of growth and another; laftly, in the larger fhells the terms are frequently wanting, which appear when they were little; fuch flender inequalities having been fince broken off by attritions.

Tho' I have folved the two difficulties propofed, yet I hardly fuppofe that they are all levelled which may be raifed on this head, and will even confefs that the figures of fome fhells are fo very fingular, that 'tis hardly conceivable how they fhould arife from a mere appofition of parts. But the folution might here perhaps be eafy, if we knew the figures of the animals which inhabit them, if the changes which befal them at feveral ages were known; and, in a word, if we could trace the growth of their fhells, with the fame care as I have purfued thofe of fnail fhells. If we did not know, for inftance, the figure of the fifh inhabiting one of the fhells, called *nautili*, and what part thereof it poffeffes, it would be impoffible to conceive how it fhould be formed by meer *juxta-pofition*. Every body knows that it is of the clafs of thofe twifted fpiral-wife, that its in-

fide

ſide is traverſed from ſpace to ſpace, by a ſort of partition all perforated in the middle. Now how can a fiſh have built all theſe partitions, and how may it be aſked can it be lodged among them? But this difficulty diſappears upon conſulting the figure of this fiſh, as given by *Rumphius*. Here we find that its body is not wreathed like its ſhell; and as thoſe of other fiſhes with wreathed ſhells are; that this only poſſeſſes the ſpace between the laſt partition and the aperture of the ſhell, that in all likelihood it is only faſten'd to the *vertex* or origin of the ſhell, by a ſtring or ligament paſſing thro' all the partitions. Thus much known, 'tis eaſy to explain how the *nautilus* forms new partitions when it is grown too big to be at eaſe in its antient nitch or ſhell, ſo as it cannot reſt on the partition without being too much crowded, and at the growth or extenſion of its ligament, allows it to change its place, it removes from the antient partition and ſits down at a proper diſtance, where having nothing to reſt the back of its body againſt, there iſſues from it a ſtony juice like that iſſuing from the body of a ſnail when any part of it is made bare. This firſt juice thickening, begins the partition, which that coming after fortifies.

The ſnail which has afforded us ſo many inſtances, will here furniſh one more, when at the beginning of winter, they retire into their ſhells, they make a kind of partition or lid thereon, which cloſes its aperture, this matter indeed is not of the ſame conſiſtence as that of the ſhell, but the reaſon is, that that part of its body turned towards the aperture, does not yield ſo ſtony juice as the reſt of its body. If the ſame ſnail continues a good while ſhut up in its ſhell, ſo that its body ſhrinks conſiderably in bulk, inſtead of one partition it forms three or four, which accordingly

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we find at a pretty diſtance from each other, and as theſe ſhew the ſeveral ſtages of the ſnail's journey or regreſs backwards; the partition of the *Nautilus* marks the ſeveral ſtages of the progreſs or journey this fiſh has ſucceſſively made forwards.

VIII. *A memoir for the conſtruction of a pump, which ſupplies the water continually into the reſervoir, by M.* de la Hire *the younger brother**.

The ſucking pump, the forcing pump, and that which is called the ſucking and forcing pump, which make the three ſorts of pumps known at preſent, give the water only by intervals; that which I here propoſe, furniſhes it continually, juſt as the double bellows make a continual wind.

This pump is both ſucking and forcing; but yet it differs from the common ſucking and forcing pumps in this, that whether the piſton riſes or falls in the body of the pump, it ſucks the water continually, and forces it alſo continually at the ſame time, which makes it raiſe double the water of a common pump, in which the diameter and height of the body of the pump, would be equal to that of the pump which I am going to deſcribe.

Let there be a body or barrel of a pump † AAA, cloſe at the bottom, and open at the top by a hole B.

C repreſents the rod of the piſton.

D the piſton. It is like thoſe of the common pumps, it is ſurrounded with a band of leather or felt, that it may be applied exactly to the inſide

* December 5, 1716. † Plate III. Fig. 1.

of the barrel of the pump; but its rod C paſſes through the hole or neck B, which is furniſhed on the inſide with a band of leather or felt, ſo that the neck ſtraightly embraces the rod of the piſton, which muſt be round, and equally thick in a length equal to the barrel of the pump.

E ſhews the ſucking tube, which is divided into two branches, one of which terminates in the upper and lateral part of the barrel of the pump, and the other at the lower part: each of theſe branches is furniſhed on the ſide of the barrel with a valve FF, which by opening into the barrel leaves a free entrance for the water of the ſucking tube into the barrel, and by ſhutting hinders its return.

G is the forcing tube, which carries the water of the barrel to the reſervoir; it is divided into two branches, which open into the barrel, and have each of them a valve HH, like the ſucking tube, but contrary to thoſe of the ſucking tube, leave a free iſſue for the water of the barrel into the forcing tube, and hinders its return. Thus the water cannot enter into this barrel but by the two branches of the ſucking tube, and cannot go out but by that of the forcing tube, for we muſt here remember, and in the ſequel of this diſcourſe, that I have already ſaid, that the hole or neck B, which is at the top of the barrel of the pump, cloſely embraces the rod of the piſton, and that conſequently the air cannot penetrate into the barrel by this aperture.

N repreſents the level of the water, which the pump is to raiſe.

Thus we ſhall perceive, that if we raiſe the piſton, the water will be raiſed in the ſucking tube, and will enter into the barrel by the lower branch of the ſucking tube, becauſe the piſton ſuſtains the column

column of air which preſſes upon it, whilſt another column of air preſſing upon the level of the water N, makes it riſe quite into the barrel of the pump. As for the air, which is above the piſton, when we raiſe the piſton the firſt time, there is no queſtion but it paſſes into the forcing tube, ſince it has no other vent. When we afterwards come to make the piſton deſcend, it is plain that the water, which has been raiſed into the barrel a little before, and is then below the piſton, will be forced into the aſcending tube, becauſe of the valve at the entrance of the ſucking tube, which ſtops the paſſage of the water from the barrel into this tube; but it muſt be conſidered, that at the ſame time that the piſton begins to deſcend in the barrel, it ſucks new water by the upper branch of the ſucking tube, for the reaſon that I have mentioned, and this water will afterwards paſs into the forcing tube, by its upper branch, when we raiſe the piſton, which at the ſame time will ſuck more, by the lower branch of the ſucking tube; which will continue in this manner alternately, as long as the piſton plays in the barrel of the pump. Thus it is evident, that by this means the piſton of this ſort of pump continually ſucks the water, and that it alſo continually forces it, whether it aſcends or deſcends; that is, that when it ſucks the water by the top of the barrel, it forces it at the ſame time by the bottom, and that when it ſucks by the bottom, it forces it by the top. And this is ſufficient to prove that this pump muſt continually furniſh water to the reſervoir, ſince there is no time loſt in the motion of the piſton, and that conſequently it will raiſe twice as much water, as any of the other pumps hitherto known.

Though the power, which makes this pump move, works continually, the advantage which

may be drawn from it amply recompenses the continual labour of the mover; for besides that this pump raises a great deal more water than another, and spares the quantity of the barrels, and consequently in the maintaining and constructing of these pumps, every body knows, that the wind and water are sufficient to make the pumps move, and that so we must make use of all the advantage that these movers furnish, since it is useless to spare them, as we are obliged to do when we make use of animals.

It is proper to mention, that it is indifferent whether the piston enters at top into the barrel of the pump, and that we may as well put it in at the bottom, if we find it convenient, as is usually practised on several occasions.

VIII. *A description of an addition to be made to windows to hinder the rain from getting into the room, when they are shut; by M.* de la Hire *the son**.

It is a very great inconvenience, when the rain gets into rooms through the windows, though they are shut, both because it spoils the frames, and because it rots the floors and cielings, and yet let us take what precaution we will, if we have only single frames to the windows, when the wind drives the rain against them, it is hardly possible but that this inconvenience must happen.

To remedy this, I have contrived to make at the lower traverse of the dormant of the windows, the addition which I am going to describe; but before I begin, it will be proper to shew what this traverse is.

* December 9, 1716.

The

The windows of dwelling houſes are of two ſorts in one the window frames run in a groove, and in the other they open like the ſhutters; it is of this laſt ſort that I ſhall ſpeak, theſe only being ſubject to the inconvenience of the rain; this ſort of window is compoſed of a wooden frame of the ſize of the aperture of the window with its *feuillures*. This frame is commonly made of four pieces of wood, of which the two upright ones are called *battant de dormans*; and the two others which join them, one at the top, and the other at the bottom are called *traverſes de dormant*, it is that which joins them at the bottom, that is now in queſtion. It is proper to obſerve, that it is upon the *battans de dormant* that the glaſs frames and ſhutters are locked.

The part* ABCHDEIL repreſents the lower traverſe of the dormant, with its ſpout cut acroſs, in the place where the two glaſs frames join on the outſide, and NOPQRS repreſents the ſection of the lower traverſe of the glaſs frame with its ſpout, and of the batten of the glaſs frame according to its height in the ſame place; BCHD is the *feuillure* cut in the lower traverſe of the dormant, and NOPQ is the ſame *feuillure* cut the contrary way, in the lower traverſe of the glaſs frame. It is commonly between the lower traverſes of the two glaſs frames, though great care is taken to make them ſhut as cloſe as poſſible, that the water gets in, and falls upon the face BC of the *feuillure*, and thence upon the inner ſupport T of the window, and afterwards into the chamber.

It is to remedy this inconvenience, that I have contrived to have the part FG of the face BC made ſloping on the ſide of the bottom of the *feuillure*, and that in the whole length of the face BC, and

* Plate III. Fig. 2.

toward

toward the extremity, and in the whole length of the ſloping face FG, from the point G to the bottom C of the *feuillure*, I have made a channel CVG four or five lines broad, and ſix lines deep, toward the middle of the window, when it does not exceed four feet and a half, becauſe this channel leſſens in breadth, as it draws nearer the battens of the dormant, where it is no more than two lines to the ſame breadth.

In the place where this channel is the deepeſt, which is over againſt the meeting of the lower traverſes of the glaſs frame, I make with a gimlet a duct VM three or four lines in diameter, which coming from the bottom of the channel GVC, goes through the *traverſe de dormant* and its ſpout DME, and ends on the outſide; this duct muſt have as great a ſlope as poſſible, and both it and the channel CVG muſt be painted with oil, and alſo the *traverſe de dormans* in its whole part FGVCHDMEI if the window is painted with oil only on the outſide.

It is eaſy to ſee by this conſtruction, that the water, which could get into the bottom BCH of the *feuillure* BCHD, cannot fall upon the ſupport T, becauſe of the channel CVG, and the ſloping face FG, and that it will eaſily go out by the channel VM, which I have found to be confirmed by experience.

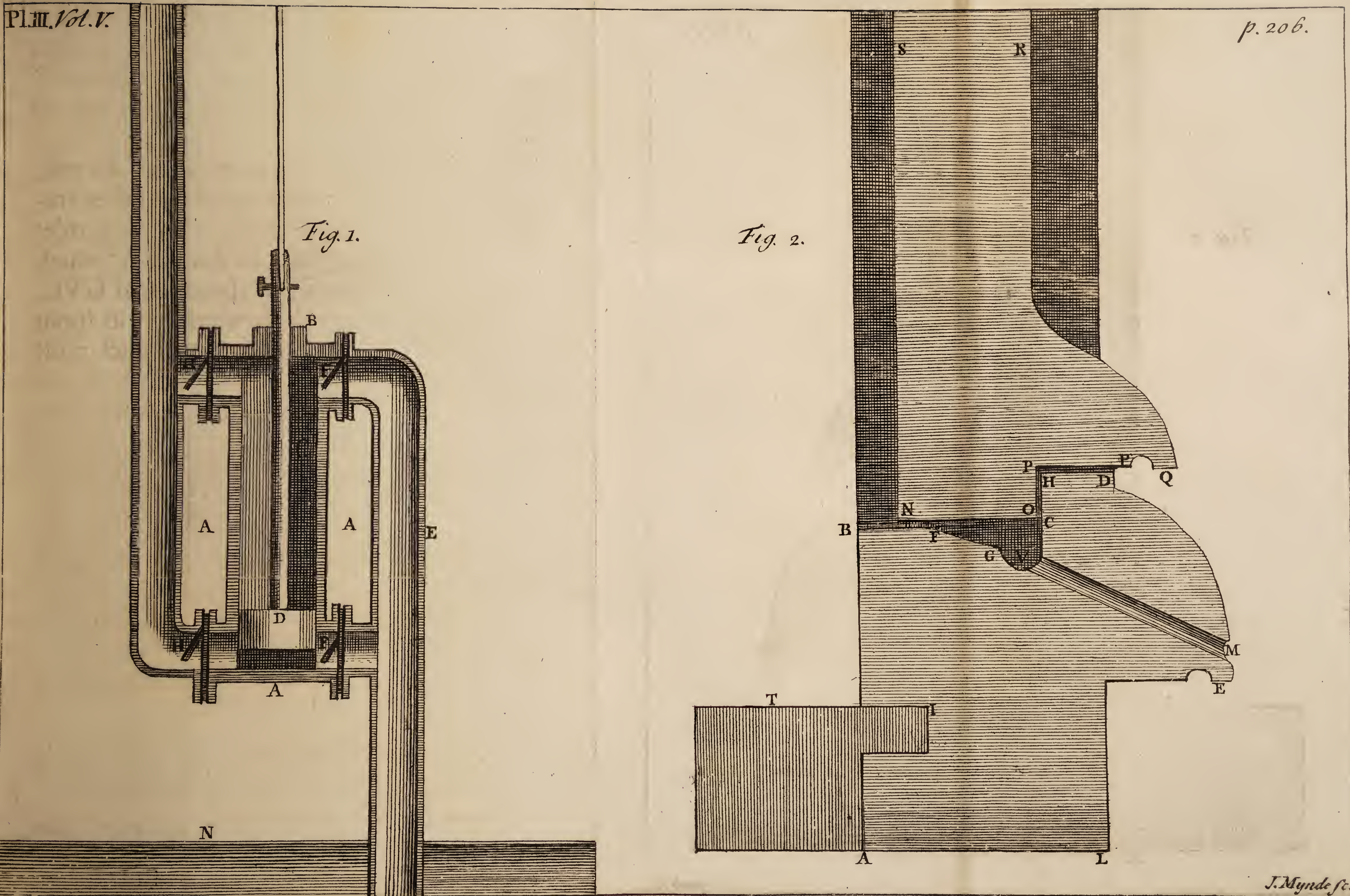
IX. *Experiments upon ſound, by M.* de la Hire; *tranſlated by Mr.* Chambers*.

The ſound formed by the meeting or colliſion of two ſonorous bodies, muſt be diſtinguiſhed from the tone thereof, which reſults from a compariſon of this to another tone of the ſame nature. The ſound of a body ſtruck, does not depend on

† Auguſt 22, 1716,

the

p. 206.



J. Mynde sc.

the vibrations of the body as the tone has been obſerved to do, but only on a tremor or jarring of the parts of ſuch body. This M. *Perrault* was apprized of, and has ſince been confirmed by me in ſomething I have publiſhed on that head, which M. *Carré* has thought fit to adopt in an expreſs treatiſe upon muſick.

The experiment I am now about to relate is very extraordinary——I took ſeveral cylinders of different kinds of wood, as well as of different lengths and thickneſſes, and laying them on my fingers by the middle, without graſping them, I ſtruck them with a hard body, or even with a piece of wood, from one end to the other, out of curioſity to learn their ſound, but I ſoon perceived that the ſound was not the ſame in every part of each piece; which at firſt I aſcribed to the conſtitution of the wood, imagining it denſer or looſer in ſome places than in others. But upon further conſideration I found that when the cylinders are ſtruck at the diſtance of about one 8th part of their length from either end, their ſound was very obtuſe compared to what it was at any other part, and even at their end, which was much the ſame as in the middle.

I afterwards repeated the ſame experiments upon rods of iron; and tho' they were not turn'd, but only forged, I could eaſily perceive the ſame thing, with this only difference, that the place where the ſound altered was much nearer the ends than in the cylinders of wood.

Not contented with this, I made another experiment with braſs wier, ſuch as that uſed in harpſichords; ſtretching a piece of this on a deal-board upwards of five foot long, five inches broad, and an inch thick, when it was brought to a moderate tenſion. I ſtruck it from one end to the other

other with the nib of a pen, but found no ſenſible difference in the ſound: however, knowing by ſome former experiments, that when a like board was placed on a ſtone pavement, and reſted by the other end againſt a ſtrong door, the ſound yielded by the ſtring, was much greater and ſtronger. I placed this in the ſame manner, and it had the ſame effect.

This experiment may ſerve to ſhew that the different ſounds of cylinders of wood or iron do not ariſe from the different vibrations of thoſe bodies, ſince in a ſtretched cord no ſuch diverſity of ſounds is obſerved, tho' there is no doubt but that its ſound ariſes from its vibrations.

X. *A continuation of the experiments upon ſound, by the ſame author* *.

In a memoir which I preſented to the academy in 1694, touching the ſounds of the ſtring of the trumpet marine, I mention'd ſeveral experiments which I had made with common fire-tongs; theſe tongs conſiſt of two flat branches of iron, which are joined together by an arch of the ſame ſubſtance, but broader than the branches. The moſt conſiderable of theſe experiments, is that of the ſound yielded by theſe branches when ſtruck with a piece of iron, and ſuſtained by the bough with a little ſtring, or on the tip of the finger, for the ſound they now produce is very clear and ſtrong, as much as the matter will admit of; whereas if the ſame bough be ſuſtained with the ſhank of a key, or on the back of a knife, the ſound will be quite deadened, notwithſtanding that the vibrations of the whole tongs are the ſame as before; a ſufficient proof that it

* Sept. 5. 1716.

is

is not theſe vibrations that form the ſound. It may be added, that a ſenſible jarring is heard in the bough, eſpecially when ſuſtained by the edge of a knife in a certain poſition, by reaſon thoſe vibrations are now interrupted. This may alſo be confirmed by faſtening a piece of lead to the extremity of each branch of the tongs; for in this caſe the clear ſound, which the tongs before yielded when ſtruck on the tip of the finger, becomes obtuſe by reaſon the jarring of the parts of the iron is deadened by the ſoftneſs of the lead.

But I have ſince made ſome other experiments, which ſeem more conſiderable than theſe, and may ſerve as a proof of this ſyſtem. When the tongs are held by their bough on the tip of the finger, and the branches are ſtruck flatwiſe with the piece of iron, we hear a very clear ſound, and the branches make ſenſible vibrations approaching to and receding from each other with great velocity: but if in the ſame diſpoſition of the tongs the branches be ſtruck edgewiſe, the ſound they yield is very clear, but much acuter than the other; and in one of theſe experiments I fancied that the tones of theſe ſounds made a fifth, tho' in reality the vibrations of the branches in the two different manners of ſtriking them, cannot be at all different as to duration, it being the ſame ſpring that is put in motion, but they are ſomewhat leſs extenſive when ſtruck on the flat ſide, ſince the motion impreſſed on them is leſs.

This experiment I repeated with an iron ruler 2 foot 8 inches long, 15 lin. broad, and 2 lin. thick, ſuſpending it by the middle on my finger, I ſtruck it on the flat ſide, and it yielded a very clear ſound; but ſtriking it again on the edge, tho' in the ſame direction, it afforded a much ſharper ſound, as in the experiment with the

tongs; and yet one would have expected the ſound to be more acute when it was ſtruck on the flat ſide than on the edge, for the vibrations muſt be equal on each ſide the *fulcrum*, and ſhorter of neceſſity than thoſe which happen upon a lateral ſtroke, as the *fulcrum* makes no oppoſition in this latter caſe; tho' it muſt be remembred, on the other ſide, that the body of the ruler makes more reſiſtance to the impulſe, anſwerably to what would be done by a thicker body.

This led me to imagine that ſomething new might be diſcovered by ſuſpending the ruler by one end with a ſmall flexible packthread, and in this poſition ſtriking it at the other end, firſt on the flat ſide, and afterwards on the edge, the effect was, that the ſtroke on the flat ſide yielded a much ſharper ſound than that on the other, contrary to what happened in the former experiment, where the ruler was ſuſpended by its middle; the part of the ruler was alſo noted where the ſound became deadened, as had before been done in the cylinders, both when the ruler was ſuſtained by its middle and by one end.

To diſcover in ſome meaſure what befalls theſe ſounds, we muſt explain the nature of that jarring, and how it is produced in elaſtic bodies; for as to the vibrations of ſuch bodies, they are ſo viſible that there is no doubt but that there are changes of the whole figure of the body.

The vibrations of a ſpringy body produce no ſenſible ſound, how great ſoever thoſe vibrations may be, as may appear in tongs, by ſqueezing the extreams of the branches againſt each other, and letting them ſuddenly looſe again; for here the vibrations will be very conſpicuous without any ſound being heard. On the contrary, upon the ſmalleſt touch of a ſonorous body with a hard one,

how

how great or heavy ſoever the body touched may be, a ſound will be heard. Now ſuch ſound can only ariſe from a jarring of the body ſtruck, which is no other than the particles of ſuch body diſlocated in that part; and communicating the ſhock ſucceſſively to all the reſt, this obliges the air incloſ'd in its pores to iſſue out; but the pores immediately recovering themſelves, and becoming even larger than before, admit a new air; and 'tis the repeated ſtrokes of this air that makes an impreſſion on the ear, and produces the ſound. It cannot however be denied, but that the vibrations of a body, from whence undulations ariſe, produce a motion in the air, lodged in the pores of ſuch body; but this motion is ſo ſlow, compared to that which cauſes ſound, that the ear cannot perceive it; and yet this motion mingling with that of the jarring, produces a diverſity in the ſounds of bodies ſtruck, ſo that it may be probably advanced, that the ſound is produced by the jarring, but that the tone of the ſound is determined by the vibrations; ſo that 'tis the different vibrations or undulations of ſonorous bodies, that make the different tones of the ſound; and the like even holds in one and the ſame body, as appears from the experiments above related. Hence alſo it is, that when the motion of the vibrations agrees with that of the jarring, we hear one diſtinct ſound, which will even make a concord with another ſound of the ſame body ſtruck after a different manner, provided the jarring were the ſame. For ſuppoſing the vibrations different, and that their relation to each other is near that of one to two, or two to three, *&c.* the jarring will conſpire with thoſe vibrations, and produce a concord. It muſt alſo be obſerved, that tho' the vibrations of ſuch body ſhould not

agree exactly with the jarrings, there will be a concord neverthelefs, fince the motion compounded of the two will agree thereto, as I have obferved in my account of the tones of the trumpet marine. Thefe confiderations may lead us to the caufe of the deadning of a found produced in a long fonorous body when ftruck in a certain place.

By what we have fhewn it appears, that by how much the matter of a body ftruck is harder and more brittle, the clearer and fhriller is the found it yields, by reafon the jarring here is more intenfe: but this does not hinder but that the found of ftrings, whether of metal, or of other firm matter well ftrained, when ftopt, may receive the fame impreffions as if they were much harder, and ftruck in the ordinary way; and in what part foever of their length they happen to be ftruck, the tone will ftill be the fame, fince their jarring is immediately communicated along the ftring, and the vibrations all reduce themfelves to thofe of the middle, where they perfift, and always remain the greateft, not in the parts where they were ftruck.

A

TABLE

OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXVII.

In the HISTORY.

In the MEMOIRS.

V.

AN

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1717.

I. *On the* Florentine *ſtones*; *tranſlated by Mr.* Chambers.

IT is an obſervation which holds almoſt univerſally in nature, that the ſame things paſs both in great and in little, ſenſibly and inſenſibly on different occaſions; and the beſt ſolutions are thoſe which transfer the cauſes diſcovered in the groſſer *phœnomena*, to thoſe which are nicer and more ſubtile. M. *de la Faye* had obſerved that the hones dug out of a quarry in *Lorrain*, where they are found over a kind of ſlate, are ſometimes intermixed with black veins which make them leſs fit for the razor, by reaſon the matter of ſuch veins being different from that of the ſtone, prevents its ſliding freely and equably over it; and yet further conceived that theſe veins which penetrate the whole yellowiſh ſubſtance of the hone, muſt come from the ſlate, which being yet fluid, as all ſtones once were, at the time when the hone hardened, had thrown a foreign matter into the ſame, which gradually inſinuated through all the clefts, or interſtices it found open; and he further imagined that the ſlate might thus be determined to riſe into the hone, by the preſſure of the ſuper-incumbant earth which

which ſqueezing one of them againſt the other, while both were in a ſtate of fluidity, had obliged that which was fluid to inſinuate into that which was leaſt ſo.

This explication which carries with it a great degree of probability, may eaſily be applied to ſeveral parallel facts, and accordingly we ſee M. *de la Faye* apply it to the *Florentine* ſtones, wherein plants, trees, caſtles, ſteeples, and ſometimes geometrical figures are plainly expreſſed; the whole of which is reſolvable into veins and ramifications of ſome foreign matter, which has inſinuated into the ſubſtance of the ſtone, as ſlate into the hone.

On this principle the moſt common repreſentation ſhould be plants, it being very natural that the foreign matter which is ſuppoſed more fluid than the ſtone, ſhould divide and ſubdivide into an infinite number of different tracts, which reſemble branches; in effect it is by no means conceivable that theſe ſhould be the impreſſion of any real plants, ſince they penetrate the whole thickneſs of the ſtone, which a plant would never do; add that they want much of that exactneſs, which a copy thus taken from its original muſt have; the pretended branches being frequently not continued but only dotted lines, not to mention that caſtles could never have left their impreſſions here.

II. *Of a magnetical ſtone brought from the iſland of* Ceylan.

There is a little ſtone, which is found in a river of the iſle of *Ceylan*, about the ſize of a *French denier*, flat, round, about a line in thickneſs; brown, ſmooth and ſhining, without either ſmell or taſte, which attracts and repells little light bodies

dies, as aſhes, filings of iron, and bits of paper; M. *Lemery* ſhewed it, it is not common, and that which he had coſt fifteen livres.

When an iron needle has been touched with a loadſtone, the loadſtone attracts the north pole of it by its own ſouth pole; and by the ſame ſouth pole it repells the ſouth pole of the needle, thus it attracts or repells different parts of the ſame body according as they are preſented to it, and it always attracts and repells the ſame.

But the ſtone of *Ceylan* attracts and repells the ſame little bodies preſented to it in the ſame manner, and it is in this that it differs very much from the loadſtone. It ſeems to have a *vortex* which is not continual, but forms itſelf, ceaſes, and begins again inceſſantly; at the inſtant when it is formed, the little bodies are driven toward the ſtone; it ceaſes, and they continue where they were; it begins again, that is, there comes out of the ſtone a new *effluvium* of matter analogous to the magnetic, and this *effluvium* drives the little bodies away. It is true that according to this idea the two contrary motions of the little bodies ought to ſucceed each other continually, which is not the caſe; for that which has been driven away, is not attracted again. But that which they would have attracted they lay pretty near the ſtone, and when it repells this body, it drives it to a greater diſtance. So that what it has once driven away it cannot any more draw to itſelf, or which is the ſame thing, its *vortex* has more force to drive away when it is forming itſelf, then to attract when it is formed.

III. *Of a globe of fire ſeen at* Queſnoy.

M. *Geoffroy* the younger, informed the academy that on the fourth of *January* at *Queſnoy*, the weather being very cloſe and the clouds hanging ſo low that they ſeemed to touch the houſes, a whirl or globe of fire appeared in the clouds in the middle of the ſquare, and with a noiſe like the report of a cannon burſt itſelf againſt the tower of the church, and ſpread itſelf upon the ſquare like a ſhower of fire, ſince which the ſame thing has happened at the ſame place.

IV. *Of an ebbing and flowing well.*

In the court of the inn on the road of *Plougaſtel*, between *Breſt* and *Landernau*, there is a well which riſes when the ſea, which is very near it, ſinks; and on the contrary ſinks when the ſea riſes. This is ſo well known in the country as a wonder, that M. *Robelin*, a ſkilful mathematician, thought it worth his inquiring into, and has ſent an account of it to the academy with a very plain explication. The bottom of the well is higher than the level of the ſea at low water in any tide whatſoever, thence it happens that the water of the well runs out as far as it can, or that the well ſinks even when the ſea is beginning to riſe, which continues until it is riſen up to the level of the bottom of the well, and afterwards as long as the ſea continues to riſe, the well riſes with it. When the ſea retires, there is yet a conſiderable time, during which a remainder of the ſea water, which has entered into the earth, penetrates it ſlowly, and falls ſucceſſively into the well, which riſes again although the ſea ſinks. This water is ſo well filtered

tered in the earth that it loſes its ſaltneſs; when this is exhauſted the well begins to ſink, and the ſea finiſhes it. As this well, which has not been digged to the ſpring, and is defended only with a wall of uncemented ſtones, and alſo receives the waters from a neighbouring mountain, when there have been great rains, we muſt have a regard to the alterations that theſe waters may bring to that which depends only upon the ſea. They hinder it from drying entirely in the winter, when the ſea is low. It dries ſometimes in ſummer for want of this aſſiſtance,and becauſe all the ſea water is drank up by too dry a ſoil.

V. *Of a remarkable ſinking of the river* Eraut.

The 6th of *June*, about 8 or 9 in the morning at *Agde*, which is toward the outlet of the *Eraut*, this river ſunk all at once, and ſo low that the bridge of boats ſunk alſo, and they were obliged to looſen the chains and cables which held it. This ſinking which appeared to the eye very conſiderable, becauſe of the ſloping of the ſhore, and the great contraction of the bed of the river, was not more than 6 feet. The common height of the water in the port of *Agde* is about 22. It remained a quarter of an hour in this ſtate, after which it began to riſe very faſt, for another quarter of an hour, it was at firſt a foot higher than it had been, but in a moment after it reſumed its firſt level. This increaſe of a foot in height was only the effect of the rapidity of its return. The ſame day in the afternoon there happened another ſinking, but leſs conſiderable. In both, when the water returned, it was ſalt, and conſequently came from the ſea.

 The

The circumſtances that attended this great ſinking, and the return of the water, were a little wind which varied from the N. E. to the S. E. and N. W. a little calm, and a ſmall quantity of rain, it had alſo rained the night before.

The water of the ſea does not mix with that of the river, neither when the wind is at N.W. and freſh, becauſe it throws the water of the river into the ſea, nor when the river is full, becauſe then it is above the level of the ſea. But when the wind is at S. E. and S. W. it drives the water of the ſea towards the river, and they call it *full*, becauſe the water is then higher than ordinary, by a foot and a half or two feet, and it is then ſalt.

The water of the ſea and that of the *Eraut* at *Agde* are commonly pretty near the ſame level. They reckon from the outlet to the bridge of boats, to be 1750 toiſes.

There is no ſenſible flux or reflux upon the borders of *Agde*.

There has been at other times at *Agde* the like ſinking. One has been known to happen in *February*, but commonly they are only in *June*, *July* and *Auguſt*, ſometimes they have been 3 or 4 times in one day. Some ſailors relate that they have at ſuch times met with ſtorms, and that there are ſeveral currents to the ſea.

It is to M. *Mairan* that the academy owes this account, it ſeems reaſonable enough to attribute this *phænomenon* to an earthquake; which muſt have been only at the bottom of the outlet of the river, and of the ſea. It muſt all at once have ſunk the earth beneath the river, and then have raiſed it, and reſtored it to its former ſtate.

VI.

VI. *Of an ox's horn which seemed to vegetate.*

The same M. *Mairan* sent to the academy an account of an ox's horn * which seemed to have vegetated in the earth, and has shewn it to the company since his being a member of it. This horn was pulled out of the earth with a plough, by a labourer near the city of *Beziers*. From the base of it proceeded a prodigious number of fibres which had the appearance of fresh, juicy and lively roots just sprouting, and at first represented a sort of vegetation. But on examining them more closely, M. *Mairan* undeceived himself, they were cylindrical, hollow and smooth on their inner surface, whence one might easily pluck the silky fibres, after letting them soak a little in water. Upon the fire they smell like burnt horn, and the chymical dissolvents have almost the same effects upon them as upon horn, all this proves that these false roots were real animal productions. Some unknown and subterraneous insects that make bags like the caterpillars, must have gathered themselves in great numbers about the horn, have been nourished by it, and have fixed their bags which they have spun and built after their manner. It follows from hence that they must necessarily have been hollow, the insects may perhaps have come out of it under some other form, as many other do which make bags only to prepare themselves for their metamorphosis, all the rest evidently agrees with this explication. M. *Mairan* acknowledges however that he has put other horns in the earth, and that there has not happened any thing like it to them, although he has varied the experiment many ways. He has not

* Plate IV. Fig. 1.

seen

ſeen either the appearance of roots or bags of inſects. Only he has ſeen that thoſe which have been left expoſed to the air, have been eaten by a hairy inſect, which is made very like a ſmall caterpillar, one or two lines in length, but has legs, and walks pretty faſt. It is the ſame animal that eats ſhells and human hair.

VII. *Of two ſtreams of light ſeen in the heavens.*

The 15th of *October*, at half an hour after 10 at night, M. *de la Hire* the ſon, ſaw a white and pretty lively ſtream of light, about two degrees in breadth, and five or ſix in height, which appeared directly to the north behind ſome thick clouds, raiſed about 15 degrees above the horizon. It was like thoſe which are formed by the light of the ſun hidden behind the clouds, when it paſſes between two different planes of theſe clouds, and meets with trains of vapours, which are illuminated by it, and expoſed to our eyes.

This ſtream of light was a minute in riſing, and ſtood ſtill during four or five more; after which it began to enlarge, and ſpread itſelf in three or four minutes into a ſpace of 20 or 25 degrees, partly towards the eaſt, and partly towards the weſt, and made all this ſpace much more light, as well as ſome clouds which were at its extremities. Afterwards this light grew fainter always from the middle towards its borders, in ſuch a manner that it was entirely extinguiſhed in the middle, whilſt towards the borders it ſtill remained pretty ſtrong.

This *phænomenon* being finiſhed, there appeared another at three quarters after 10, in the ſame

ſame place, quite like the former, both in figure, progreſs, and end.

It is eaſy to ſee, by the ſituation of the ſun at that time, that it had no ſhare in theſe effects, nor can we impute it to the moon, altho' it was 11 days old, and pretty high in the horizon. The extent of theſe lights, their ſucceſſive increaſe and their manner of decreaſing, can only agree with exhalations that had inflamed themſelves in the air; and that ſo juſtly, that it is eaſy to ſee we cannot ſearch for another cauſe. Theſe lights are in little what the horizontal are in great.

AN

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1717.

I. *Meteorological observations made at the royal observatory, during the year* 1716; *by M.* de la Hire*.

THE water, which fell in rain or melted snow, was in height during the month of

	Lin.		Lin.
Jan.	29 $\frac{1}{8}$	July	24 $\frac{2}{8}$
Feb.	9 $\frac{7}{8}$	Aug.	3 $\frac{7}{8}$
March	10 $\frac{3}{8}$	Sept.	27
April	6 $\frac{2}{8}$	Oct.	27 $\frac{1}{8}$
May	10 $\frac{3}{8}$	Novem.	10 $\frac{4}{8}$
June	4 $\frac{3}{8}$	Dec.	8 $\frac{4}{8}$

The sum of the height of the water of the whole year 1716, is 172 lines $\frac{1}{4}$, or 14 inches, 4 lines $\frac{1}{4}$.

This year was very dry in proportion to the middle years, in which we have established that there fell 19 inches of water; and as there fell but little water in the spring and in the summer, the hay and summer corn produced but little, and the greatest part of the fruit did not succeed, but withered upon the trees without ripening, in the provinces where it is usually the most plentiful. Neverthelefs the wheat harvest was very good,

* Jan. 9, 1717.

for

for the drought which had done harm on one ſide, was very uſeful towards the cleanneſs of the grain, which was not choaked by the weeds which uſually grow in wet years, and cauſe the corn to ſhed; beſides, the ſtraw was very ſhort, and the ear was well ſupported. The ſummer months which commonly ſupply a great deal of water by ſtorms, have afforded but very little; and this is what makes this year ſeem very dry in proportion to the middle ones.

It ſnowed very much during all the month of *January*, and the greateſt ſnow was the 31ſt of this month, which being meaſured ſupplied 16 lines of water; the wind was then S. W. Alſo this month has furniſhed more water than any of the other.

The ſnow in *December* was not great.

There were no ſtorms this year, only a pretty large clap of thunder the 19th of *September*, with a moderate rain, by a ſtrong S. W. wind.

The great quantity of very thick fogs which were this year, have been of great ſervice to the plants and trees.

The winds were commonly pretty variable; but on the 31ſt of *October*, there was a very violent ſouth wind, and a rain of almoſt 6 lines. The laſt day of *November* the wind was pretty ſtrong at S. W. but without rain.

On the thermometer.

My thermometer, which I have always uſed, fell at the loweſt to 4 parts $\frac{2}{3}$ the 22d of *January*, which is a ſign of very great cold, but it laſted only the night before, for the following days it roſe again conſiderably. One may judge of this cold by comparing it with that of *January* 1709, which was reckoned one of the greateſt that had

 been

been known in this country, for the ſame thermometer fell only to 5 parts the 13th and 14th of *January*, and the 21ſt to 5 $\frac{3}{4}$; alſo the cold of the year 1709 laſted longer than that of this year. The wind on the 22d of *January* this year 1716, was north, but that on the 13th and 14th of *January* 1709, was at firſt very gentle; then middling at N. N. W. and at laſt turned to the W. and became very ſtrong, with ſnow. In the beginning of the month of *February* this year, when it is often very cold, it hardly froze.

This ſame thermometer roſe very high at the end of the month of *July*, and all the month of *Auguſt*; and it was at the higheſt on the 22d of *July*, at 62 parts $\frac{1}{2}$ with a middling weſt wind, and at 63 parts the 23d of *Auguſt*, with a middling ſouth weſt wind.

All theſe obſervations were always made towards the ſun-riſing, which is the coldeſt time of the day; and about two or three hours after noon it generally roſe again to 12 parts, at leaſt in the ſummer, for in the winter it riſes very little.

On the barometer.

My common barometer, which is always placed at the top of the great hall of the obſervatory, fell at the loweſt to 26 inches 9 lines $\frac{1}{2}$ the 1ſt day of *January*, with a middling ſouth wind, and it roſe at the higheſt to 28 inches 3 lines the 16th of *February*, with a north-eaſt wind; therefore the difference between theſe heights was, as it generally is, very nigh 17 lines $\frac{1}{2}$. I obſerve here, as I have already in many years, that the barometer is commonly low when the wind comes about the ſouth, and is high when the wind gets towards the north; and that it often rains when it is low, and is fair when it is high. But there are great

great changes in the winds, which may confiderably alter this obfervation, for the winds which we obferve, are only winds that reign upon the furface of the earth: befides, we often fee when there are clouds very high above thofe which are near the earth, that a low wind comes from a quite oppofite fide to that which reigns above, and that might be without being perceived, if there were no clouds in the upper part of the air to make us obferve it. It might alfo be, that thefe two oppofite winds might deftroy each other, and caufe a calm. But to make thefe obfervations a little juft, we ought to be aware whether the fame wind has reigned for fome time; for a fudden wind, which forms itfelf upon the furface of the earth, makes no alteration in the height of the atmofphere, which is fhewn us by the barometer; and it is this height of the atmofphere which fhews us moft juftly rain or fair weather: for we know that in the northern parts of the earth, the atmofphere is a great deal more raifed than in the fouthern, and that there rifes much fewer vapours in thofe countries than in thefe: wherefore the winds which come from the northern countries are always drier than thofe which come from the fouthern; and that thefe northern winds increafe the height of the atmofphere; and on the contrary the others diminifh it. Thus the finking of the mercury in the barometer muft fhew rain, and its rifing fair weather.

We may ftill add to this, that when the atmofphere is lefs heavy, and confequently lefs condenfed, which is equivalent to a more rare *medium*, it cannot fuftain the particles of vapours that fpread upon it, which are obliged to fall, and fo form rain. On the contrary, when the atmofphere is more condenfed, it approaches nearer

to a more denſe *medium*; which ſuſtaining theſe vapours, does not occaſion any rain; alſo the ſtinks which exhale from the common ſewers, are ſmelt very ſtrong when the barometer is low, but they eaſily riſe in a thick atmoſphere, which drives them up, as we obſerve in ſmoak.

On the declination of the needle.

We have obſerved the declination of the needle with many needles of different lengths and conſtructions. The firſt is quite a plain needle of a thread of ſteel eight inches long, placed in a wooden box; it gave us the declination of 12° 20′ of N. towards the W.

The ſecond needle was alſo a thread of ſteel 13 inches ½ long, placed in a box of *Liais* ſtone, which we have deſcribed in the memoirs of the preceding year, and this gave us the declination of 11° 45′.

The third was a needle like the preceding, of 13 inches ½, and placed in the ſame box; and this gave us the declination of 12° 20′, like that of eight inches long.

But having charged this needle with two little pieces of ſteel, long and pointed at both ends, with the points anſwering to thoſe of the needle, to ſee whether this ſort of needle, which is in a manner only two little pieces of loadſtone joined by a thread, when the whole needle has been touched, would not change the direction of the needle from what it was before it was charged. It then gave us the declination of 13° 25′.

We know then hereby, that this needle being thus formed or charged, has augmented the declination more than a degree, which may make us ſuſpect that theſe ſort of needles which are made like arrows, and have at their extremities

two

two pieces much thicker than the thread which joins them, may cauſe different declinations, by the nature of the ſteel of theſe pieces which may have been variouſly touched. Alſo this ſort of needle in appearance ſeems more moveable than the plain ones, by the vibrations that are made by theſe two pieces which are added.

The 9th of *May*, at five in the morning, I obſerved a *parhelion* toward the ſouth, with regard to the ſun; it was between light clouds, and of the ſame height as the ſun, and the centre of the *parhelion* was $22^{\circ} \frac{1}{4}$ diſtant from the centre of the ſun; its red colour was turned towards the ſun, and the blue the contrary way. We have elſewhere obſerved, that the diſtanee between the centre of the ſun and that of the *parhelion*, differs according to the denſity of the air, or to heat and cold: the time of its appearance is commonly in *May*.

II. *Obſervations on a horizontal light, by M.* Maraldi *.

At the end of the year 1716, and beginning of 1717, there appeared in the heavens for many nights a great horizontal light, whitiſh and reſembling the twilight. We obſerved it the 15th and 16th of *December* laſt, and the 6th, 9th, 10th and 11th of *January*, having been viſible ſix times, twice in *December*, and four times in *January*.

The 15th of *December*, after having obſerved the entrance of the fourth ſatellite of *Jupiter*, and the ſhade of the ſame ſatellite, which juſt touched the ſouthern edge of the planet, at half an hour after 10 at night; that is, five hours after the

* March 10, 1717.

end

end of twilight, we ſaw the heavens light towards the N. and N. E. as if the moon had been juſt going to riſe at that place. It had however been ſet above four hours in the oppoſite part of the horizon, ſo that it could not be the cauſe of this appearance.

The light was extended over the ſtars of the conſtellation of the *Lion*, which was its fartheſt eaſtern bound; it paſſed below the ſtars of the *Great Bear*, and ended near the head of the *Dragon*, which was toward the N. W. filling in this ſpace of the heavens from N. E. to N. W. near 90 degrees of a great circle. It was terminated on one ſide by the horizon, whence it ſeemed to come out like the twilight, and raiſed itſelf to the height of 10 or 12 degrees, for it touched, with its upper edge, the bright ſtar in the tip of the *Great Bear*'s tail, and paſſed near the ſtars in the head of the *Dragon*. We had time to obſerve it only for a quarter of an hour, the heavens being covered at $\frac{3}{4}$ after 10.

The 16th of *December*, at half an hour after nine, the heavens being clear again, the light was viſible, and paſſed, as the night before, below the ſtars of the *Dragon*, and through thoſe of the *Great Bear*, being terminated on the eaſt by ſome clouds which covered the ſtars of the *Lion*; but theſe clouds being afterwards diſperſed, the light appeared upon theſe ſtars alſo; ſo that they appeared in the ſame ſituation as they did the preceding night, and had the ſame extent. Afterwards, the heavens being partly covered, we could not continue to obſerve the bounds of the light, but we ſaw, till half an hour after midnight, through the openings of the clouds, the part of the heavens from the N. E. to the N. W. illuminated in theſe openings with the ſame force that it was

was before, when the heavens were quite clear. We obſerved alſo that theſe clouds, in changing place, hid the lights from us in the places of the heavens through which they paſſed, and that the light appeared again in the ſame places, which the clouds afterwards quitted. This ſhews that the matter, which was the cauſe of the light, was farther diſtant from the earth than the clouds were, ſince they hid the light from us when they paſſed over that part of the heavens where it was viſible, and permitted us to ſee it when they quitted the ſame places.

After the 16th of *December*, the heavens having been covered for ſome days together, and the light of the moon ſucceeding, we were not able to ſee theſe lights till *January* 1717.

The 6th of the ſame month the heavens were covered all the day, and were not cleared till towards $\frac{1}{4}$ after 10 at night. We then ſaw the light was very bright like the twilight, and it encompaſſed the horizon. The ſtars of the *Dragon's-Head*, which were in the lower part of the meridian on the north ſide, ſeemed a little plunged in the light, and continuing toward the eaſt, it paſſed near the weſtern ſhoulder of *Boötes*, below *Berenice*'s hair, and the bright ſtar in the tail of the *Lion*, through the ſtars of the *ſhip*, and thoſe of the great *Dog*, which were near the meridian on the ſouth ſide. From thence it was ſeen in the ſtars of *Eridanus*, on the belly of the *Whale*, which was in the weſt; and then paſſing towards the N. W. through the two bright ſtars on the breaſt of *Pegaſus*, and the extremity of his wing, and through the bright one in the tail of the *Swan*, it finiſhed the whole circumference with the ſtars of the *Dragon*.

This

This light was bounded by the horizon, as in the preceding obſervations; but it was not equally large every where. There were two places almoſt oppoſite to each other, where it roſe to the height of about 20 degrees; for on the N.W. ſide it touched with its upper edge the moſt ſouthern ſtar in *Cepheus*; and on the S. E. ſide, near the ſtars of the *ſhip*, it roſe as much, and was brighter than the milky-way which was near it. In the reſt of the horizon, the light roſe only about 12 degrees, and it was equally bright every where, except toward its upper edge, where it loſt itſelf inſenſibly. It had alſo a little ſpace on the S. W. ſide, where it was leſs bright than the reſt of its extent. Some clouds which touched the horizon interrupted it a little in the lower edge, but it roſe every where much above theſe clouds; ſo that in its upper part it was continued and encompaſſed the horizon.

We continued to ſee it in this manner till half an hour after midnight, when the clouds hid it from us entirely, together with the heavens.

The 9th of *January*, after the going of the ſecond ſatellite out of the ſhade of *Jupiter*, which was a little before eight at night, the heavens being partly clear, we ſaw the light pretty bright in many places of the horizon, where there were no clouds.

At midnight, the clouds being diſſipated, the light was very bright. Towards the W. we ſaw it extended over the head of the *Whale*; it paſſed through the ſtars of *Andromeda*, which were to the N.W. and through the loweſt ſtars in *Cepheus*; the bright ſtar of the *Harp* appeared in the middle of this light; the ſtars of the *Dragon*'s-*Head* were a little plunged in it. On the ſide of the N. and E. the *Crown*, *Arcturus*, the two moſt ſouthern

ſouthern ones in the girdle of the *Virgin*, were within it, and it was terminated by the ſtars of the ſhip: thus it went almoſt round the horizon. We obſerved, from half an hour after midnight till one, the entrance of the firſt ſatellite of *Jupiter*, and the immerſion of the 4th into the ſhadow.

At one in the morning, the heavens being perfectly clear, the light continued to appear very bright almoſt over the whole horizon, *Arcturus* and the ſtars in the girdle of the *Virgin*, which at midnight were plunged in the light, were ſeen at its upper edge, although it was always at the ſame height; which ſhews that it was fixed to the horizon, and did not partake at all of the motion of the ſtars from E. to W.

The ſame night after the emerſion of the fourth ſatellite, and of the firſt from the ſhade of *Jupiter*, obſerved at 20 minutes after 3, the light appeared continually very bright. It had ſtill the ſame breadth, extent, and ſituation, with regard to the horizon; but it was among very different ſtars from thoſe where it had been at midnight. On the eaſt ſide it paſſed through the ſtars of the *Swan*, thoſe of the harp, and the head, and weſtern ſhoulders of *Serpentarius*, and the ſtars of the *ſcales*. On the W. ſide it was extended over the ſhoulders of *Orion*, the head of the *Bull*, and the conſtellation of *Caſſiopeia*; ſo that, in this laſt obſervation, the light paſſed through more eaſtern ſtars than thoſe where it was at midnight, theſe being more to the weſtward; which ſhews again, that it did not partake at all of their motion, and that it was fixed with regard to the horizon, and conſequently that it was not far diſtant from the earth; for all that appears in the heavens without partaking of the univerſal motion from eaſt

to weſt, in the three hypotheſes of *Ptolomy*, *Copernicus*, and *Tycho*, is reckoned to belong to the earth, and to be in its atmoſphere. Thus alſo the matter, which was the cauſe of this light, was farther diſtant from the earth than the clouds, which were in the air at the ſame time, as we have before obſerved, it was nevertheleſs contained in the atmoſphere.

The 10th of *January*, 7 at night, the light was very clear, and encompaſſed almoſt the whole horizon. It was only on the S. W. ſide that it did not appear very ſenſibly. Some clouds afterwards riſing above the horizon, the light was partly effaced by them; but we always ſaw its upper edge when the clouds did not come at all.

The 11th of *January* the light was ſenſible only toward the eaſt; it began below the weſtern ſhoulder of *Bootes*, paſſed under *Berenice's* hair, croſſed the weſtern ſtars of the *Virgin*, where it was bright, and ended near the ſtars of the *ſhip*.

After the 11th of *January*, the heavens having been for ſome days covered, and the light of the moon ſucceeding, we were not able to make other obſervations.

It appears by what I have juſt related, that the brightneſs and extent of the light was ſubject in a little time to great variations; for the two firſt days of its appearance, which were the 15th and 16th of *December*, it filled 90 degrees, and then diſappeared; it appeared a new the 6th and 9th of *January*, four times as large as before, ſince it encompaſſed the horizon; the 10th it had diminiſhed a little toward the S. W, the 11th a great deal more, appearing bright only toward the E. upon the ſtars of the *Virgin*, and at laſt it was entirely diſſipated without having been ſince viſible.

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This light has ſome ſimilitude with that which we obſerved in *April* the laſt year, although it differs in ſome circumſtances, they agree in this, that both appeared at the horizon like twilight, whitiſh and tranſparent, ſo that we ſaw the ſtars which were plunged in it. That in *April* appeared toward the N. and N. E. filling about 80 degrees. That of the 15th and 16th of *December* laſt, appeared on the ſame ſide of the horizon having an extent of about 90 degrees; but in the greateſt part of the obſervations of *January*, this extent was much greater, ſince it encompaſſed the horizon,

In *April*, beſide the conſtant light in the form of twilight, as in that of *December* and *January*, there were flaſhes of light which roſe ſucceſſively from the horizon, and traverſed the horizontal light which did not happen in any of the laſt obſervations.

That in *April* appeared in the ſame night only for two hours, that is, from half an hour after ten and then diſperſed; the laſt were viſible all night, when the heavens were clear, and to judge of it by its brightneſs on the other nights, there is room to believe that if it did not laſt ſo long, it was becauſe the clouds hid it from us.

Notwithſtanding theſe *phœnomena* are rare, yet there have been ſeen formerly ſome which have born a reſemblance to thoſe which we have obſerved.

* *Pliny* ſays that under the conſulſhip of *C. Cæcilius* and *Cn. Papyrius*, which was 111 years before Chriſt, a light was ſeen in the night-time, which was obſerved ſeveral other times, as if a ſort of day had ſhone in the night.

† *Seneca* giving an account of ſeveral lights that appeared in the air, ſays that ſome of them are

* Lib. II. Cap. 33. † Quæſt. nat. Lib. I.

permanent in certain places, and give ſo much light, that they diſſipate the darkneſs and appear like day. He adds that among theſe there are ſome which are ſeen in the clouds, and others that appear above the clouds.

Our light was alſo fixed, and had no motion with regard to the horizon, it had beſide this particularity of appearing above the clouds, and although it was not ſo great as to repreſent the day, it nevertheleſs ſeemed like the break of day. *Calviſius* in 992, relates that on *Chriſtmas*-night there appeared to the N. ſo great a light that it repreſented the day.

A *Saxon* chronologiſt that was publiſhed by M. *Leibnitz*, ſays he ſaw in 993 the night of *St. Stephen*, ſo great a light to the N. that one would have thought the day was going to break. It appeared at the beginning of the night, and laſted for an hour.

M. *Gaſſendi* affirms that he ſaw, five different times, the northern lights, of which he has only given the deſcription of that which was viſible in *September* 1621, and which ſeems to be more like that which we obſerved in *April*, than theſe laſt.

The obſervation related by *Calviſius* and that of the *Saxon* chronologiſt have this reſemblance with ours, that they appeared pretty near the ſame ſeaſon of the year, and toward the winter ſolſtice, whereas thoſe of *Gaſſendi*, were viſible in different ſeaſons, that is 3 in *September*, one in *April*, and the 5th in *February*.

After the obſervations of *Gaſſendi*, we have had no other but thoſe of 1707 made at *Copenhagen*, by M. *Roemer*, and at *Berlin*, by M. *Kirchius*; although in this interval there had been many excellent

excellent aſtronomers, very attentive to celeſtial obſervations.

In the regiſter of the obſervations of *April* 4, 1695, after many obſervations of the ſatellites of *Jupiter*, which ſhow that the heavens were clear that night, I obſerved that toward the S. W. there were very bright clouds from the horizon to about 4 or 5 degrees in height, and 10 or 12 in length, which were terminated on the ſide of the zenith, by a black cloud. This *phænomenon*, which I obſerved from 10 at night to 11, and which appeared as if a great fire had enlightened the clouds, had rather ſome reſemblance to that which appeared in *England*, in *March*, than to our laſt, which we never ſaw better than when it had no clouds about the place where it appeared.

All the appearances of this light, obſerved by *Gaſſendi*, were followed as he teſtifies, by mild and clear days. M. *Roemer* after having given the obſervations which he made in 1707 of two northern lights, adds, that theſe *phænomena* are rather a ſign of the preſent ſtate of the air, than of that which is to follow, and that it does not always happen as ſome believe, that in ſummer they are followed by fair weather and in winter by cold. M. *Kirchius* in the obſervations which he made at *Berlin*, the 6th of *March*, 1707, ſays, that this appearance was attended by fair weather, and that it thawed all the day.

The *Engliſh* accounts which relate the obſervations of the *phænomenon* of the 17th of *March* 1716, take notice at the ſame time that the air was mild, and even hot that day ; it was alſo the ſame the 11th, 12th, and 13th of *April*, in which days we obſerved the northern lights in 1716. The air was alſo very mild during the laſt appearances of our light, the 15th and 16th of *December*, and in *January*

ary when it was viſible, which is not uſual in that ſeaſon.

It appears then, by the obſervations that we have juſt related, that the appearances of theſe lights have been accompanied with a mild and temperate air, even in winter and in cold climates, which gives room to believe that theſe lights are cauſed by ſubtile and ſulphurous exhalations, which being raiſed from the earth and kindled in the air, have helped to make it mild; for there is no room to doubt but that the ſtate of the air, in the ſame ſeaſon, is diverſified by the different exhalations; and that thoſe which kindle in the air may moderate it even when it is leſs warmed by the rays of the ſun.

It may alſo be remarked, that the years in which we have obſerved theſe laſt *phœnomena*, the rains have not been great in this climate; for in 1706, which was the year before the appearance of the light obſerved in *Copenhagen*, and *Berlin*, the beginning of *March* 1707, according to the obſervations of M. *de la Hire*, there fell only 15 inches and a half of rain, and the two firſt months of 1707 there fell but one inch, which is a ſign of drought; and during 1716, that the light was obſerved three times in *England*, and twice at *Paris*, there fell only 14 inches of rain, which is alſo a ſign of great drought. We do not know whether the ſame thing happened at the time of *Gaſſendi*'s obſervations, becauſe they did not at that time meaſure the rain that fell.

Theſe lights may probably be viſible only in dry weather, and rather the conſequence than prognoſtic of it. For in a dry year the exhalations muſt be in greater abundance, and leſs mixed with humidity, and thence are more eaſy to be inflamed

inflamed, and to produce theſe appearances of light which we have related.

III. *Deſcription of a machine* to *raiſe water*; *by M.* de la Faye.

M. *Perrault* in his tranſlation of *Vitruvius*, *L.* 10. *C.* 9. propoſes ſeveral machines for raiſing water, and in the firſt place the *Tympanum*. This machine does not raiſe the water very high, but it raiſes a great quantity of it. They make an axle rounded exactly, and ſhod with iron at both ends, which croſſes a *tympanum* made with planks joyned together, and the whole is placed upon two ſtakes, whichhave iron plates at each end to ſupport the ends of the axle. In the cavity of the *tympanum* are put 8 planks acroſs from the circumference to the axle, which divide the *tympanum* into equal ſpaces; the fore-part is made with other planks in which there are apertures made of about half a foot to let the water enter within, the length of the axle is alſo hollowed into a channel on the right of each ſpace, which go the length of one of the ſides of the axle. All this having been pitched as the ſhips are, the machine is turned by men, and then it draws the water through the apertures that are at the extremity of the *tympanum*, and diſcharges it through the channels that are at the extremity of the axle. Theſe are the words of *Vitruvius*; all the other machines of this kind, that are called *tympanum*, which I have ſeen in the *Theatrum Machinarum* of *Bocterus*, and in *Ramelli*, as alſo in ſome *Italian* and *German* books, which treat of machines, have all the common fault of raiſing the water by the *Radius* of the circle, and differ in nothing from *Vitruvius*'s *tympanum*, being

ing drawn from the ſame principle, and therefore it will be needleſs to give the deſcription of them. From hence we may conclude that this machine has conſiderable faults, the firſt and the greateſt of which is, that it raiſes the water in the moſt diſadvantageous ſituation poſſible, ſince the weight is always at the end of the *radius*, which is the longeſt lever of the circle, and thereby wearies the power which acts at a great diſadvantage and without uniformity, which is probably the reaſon why it is no longer uſed; add to this that the machine is heavy and maſſy as may be ſeen by its conſtruction, and the very exact deſign that M. *Perrault* has given of it. I muſt mention in this place that after having executed this machine, M. *de la Hire*'s *treatiſe of mechanicks* falling into my hands, I found in the 116th propoſition of this ingenious work, a conſtruction of wheels, the *arbors* of which have arms or wings to raiſe piſtons, like thoſe of the mills for gun-powder, paper, fulling and forging. Although M. *de la Hire*, to make the motion of the piſtons equal, makes uſe of a curve line, which he calls *epicycloid*, of which the circle of the *arbor* is the baſe, and is formed by a right line which turns about, and is applied ſucceſſively to all the points of the circle; as it did not appear to me in any part of his treatiſe, that he had thought of making uſe of channels to raiſe waters, by introducing them there, I made uſe of a ſmall mechanical curve, of which I am going to give a conſtruction, the ſame that I had invented and executed long ago; and afterwards I ſhall explain the properties of it.

The machine * that I propoſe has no conformity with the *tympanum* but in emptying itſelf at the centre, otherwiſe the conditions are very dif-

* Plate IV. Fig. 2, 3, 4 and 5.

ferent.

ferent. It ſeems to me to be ſimple, and as light as poſſible for its ſize. The conſtruction of it is eaſier and cheaper than its figure ſeems to promiſe, if we may depend upon the workmen, and eſpecially on two of the moſt able ſhipwrights in *France.*

It is compoſed of an iron *arbor* which paſſes through a nave which is divided into four ſeparations, over againſt the apertures of four channels, which are bent according to the moſt advantageous conditions poſſible. The machine is ſo plain, at firſt ſight, that I am afraid of waſting time in ſaying, that as the wheel turns it makes the water enter into its channels, which always remains in a fixed place, and perpendicular to the curve, upon which it acts with great uniformity, and very little effort. The following is the generation of this curve.

I took the nave of the machine, and after having ſurrounded it with a ſoft and flexible watch-ſpring, I fixed one end of it, and unfolding the other armed with a point, it formed a mechanical curve, which had for its evolution the circle of the nave. If we give to the channels the curvity that we have juſt deſcribed, it is viſible that the weight will always be in the moſt advantageous ſituation poſſible, ſince it riſes vertically by a tangent line to the nave, and which conſequently is diſtant from it only the ſemi-diameter of the nave. In this machine the power is to the effect, as the circumference of the nave or of the *arbor* is to its *radius:* for example, in this model the *arbor* is ſix inches in diameter, and conſequently about nineteen inches in circumference, of which I reckon but 18 in length for the lever, where the power is applied, becauſe of the plunging of the pallets into the water,

while the weight makes no effect but upon a lever of 3 inches, which is the radius of the nave: from whence it follows, that the power is to the effect as 3 is to 18, or as 1 to 6, and a little more.

By this conſtruction the weight to be raiſed makes always uniformly the ſame effect, which is the leaſt poſſible while the power applied the moſt advantageouſly that it can, acts with energy by the pallets placed at the extremity of the *radius* of the wheel. Theſe two conditions being anſwered, make the greateſt perfection that can be deſired in a machine, without reckoning ſome conſiderations that are not contemptible in mechanicks, which are that it has only the ſingle friction of the axis, which is neceſſary and conſequently inevitable; and that we do not make uſe of materials that are periſhable or ſubject to reparations by the friction, as of leather, with which they make the valves, and ſurround the piſtons of pumps, nor of dear and heavy materials, as metals, the whole being of wood. The perfection and ſimplicity of the machine, frees it from all theſe additional expences, add to this that the vertical elevation is the ſhorteſt. It ſeems to me to be preferable to the ſcrew of *Archimedes*, which is inclined, and diſcharges only a very little part of its water, and remains charged with the reſt, which is very conſiderable, eſpecially when it is of a great bulk, as it always is when they would draw any uſe from it, it being hardly of any ſenſible effect when it is ſmall, whereas this machine diſcharges all its water at each turn of the wheel. I do not know that any body till now has purpoſed the raiſing of waters in curved channels, according to the conditions here above expreſſed. This machine would be very uſeful

in

in places where it ſhould be neceſſary to raiſe a great quantity of water; it would eaſily furniſh a ſufficient quantity to turn a mill, and to water meadows and gardens. This advantage renders it ſuperior to all other machines yet known, for great and vaſt projects, for the communication of rivers, from which it might furniſh water to the canals which ſhould join them, for drying overflowed grounds, and for a number of other caſes.

There remains an inconveniency to this machine, which is the raiſing the water only at its ſemi-diameter. If there ſhould be occaſion for a greater height, I believe they might make uſe of two or three wheels one upon another; a means, which tho' inconvenient, would however be very practicable by the perfection of the machine, where there is neither water nor force loſt; and becauſe the power is always applied the moſt advantageouſly poſſible with relation to the weight, as has been above proved.

As to the mechanical conſtruction, as all the difficulty is reduced to bending long planks of deal or oak, or other proper wood, according to this curve, which is very eaſy to follow, as we have juſt ſeen, by the unfolding a meaſuring chain which ſhould have ſurrounded the nave. The common carpenters told me, and I ſaw that they gave the planks what curvity they pleaſed by ſtraining them with ſeveral iron or wooden pins, or by charging them with ſeveral weights; after which they make uſe of fire, which they kindle underneath, or at the ſide of theſe planks, which make them always preſerve their curvity. When they make uſe of green wood, they bend it, and let it dry without uſing of fire, which produces the ſame effect. As for the ſides, they make

them of all ſorts of planks; after which they caulk and pitch the machine; when it leaks, the leaſt caulking remedies it with eaſe, and the leaſt expence, as every body knows, which renders the work durable. This machine is neither of a great price, nor difficult conſtruction, becauſe the parts have always neceſſarily a conſtant relation between themſelves, ſo that the given thickneſs of the nave determines the diameter of the wheel, and the given diameter renders in its turn the thickneſs of the nave. This harmony which naturally proceeds from the ſubject, ſpares a trying, which almoſt always accompanies the undertakers, and diſcourages them, or at leaſt retards the work.

We might make uſe of this principle to make *Clepſydræ* more juſt than thoſe that we have, all which want uniformity.

As for the aſſemblage, if this is not ſufficient to give the ſolidity requiſite for this wheel, when it ſhall carry ladles or pallets, we may have recourſe to the ſkill of our carpenters, who are very well verſed in this affair, and they will help us in what is proper to be done.

IV. *An inquiry into the dates of the invention of the micrometer, pendulum clocks, and perſpective glaſſes, by M.* de la Hire*.

As I have perceived that in the aſſemblies of the academy, it has often been debated who were the firſt inventor of the micrometer and pendulum clock, and that each endeavours to give the preference to thoſe whom they are moſt intereſted for, I imagined it would pleaſe the academy and

* June 23, 1717.

the

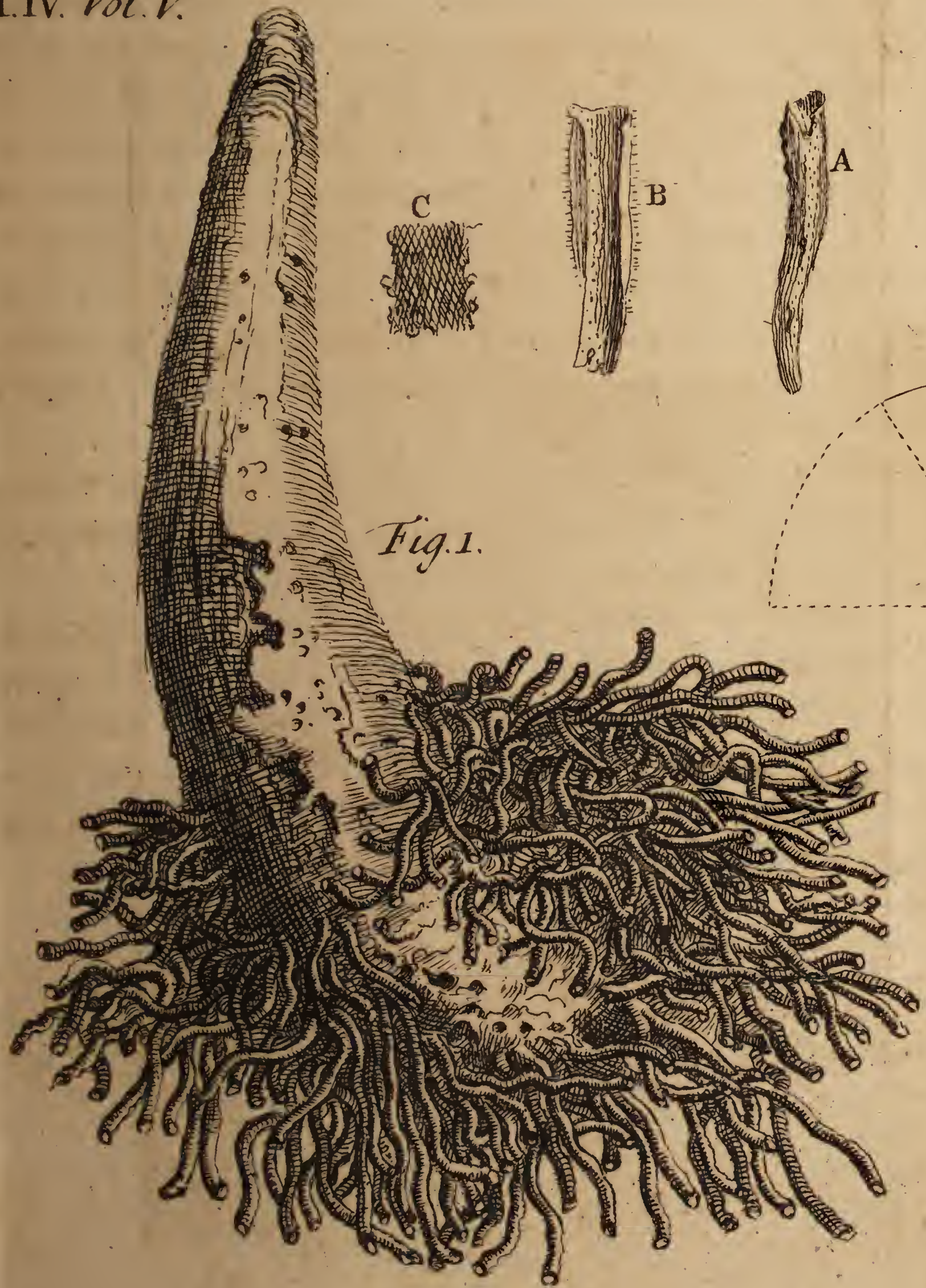


Fig. 1.

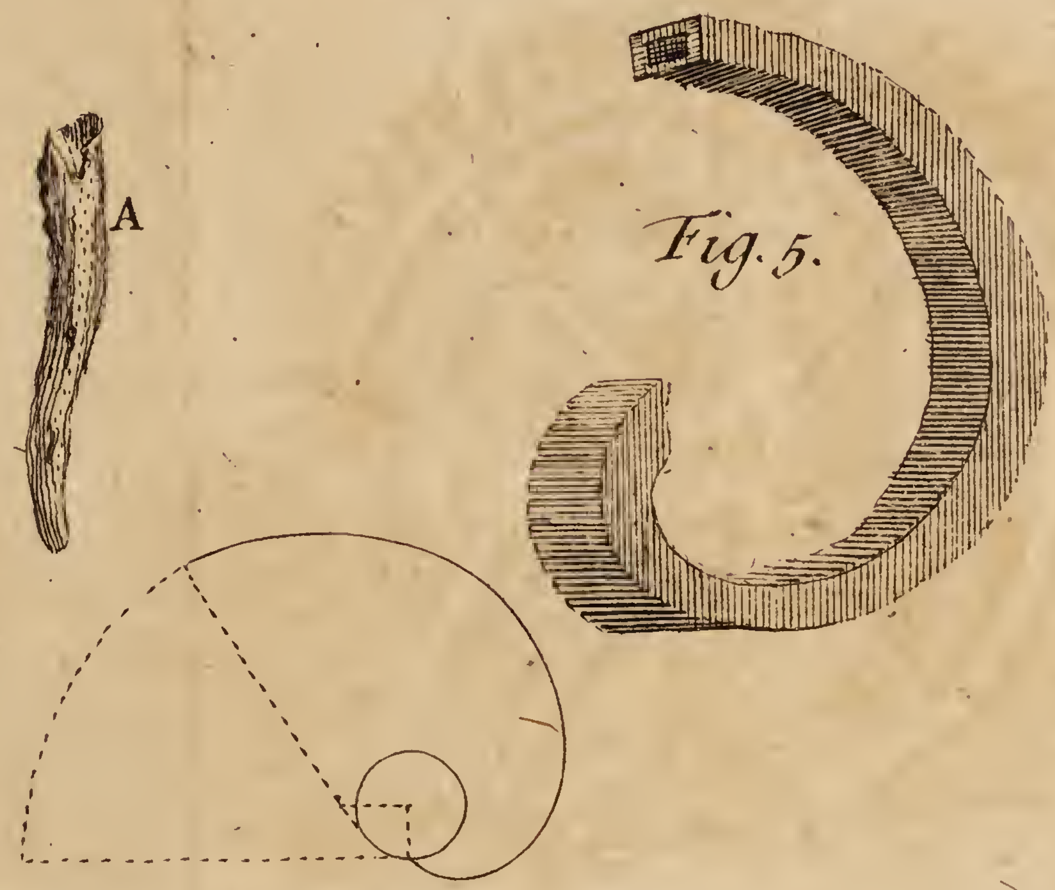


Fig. 5.

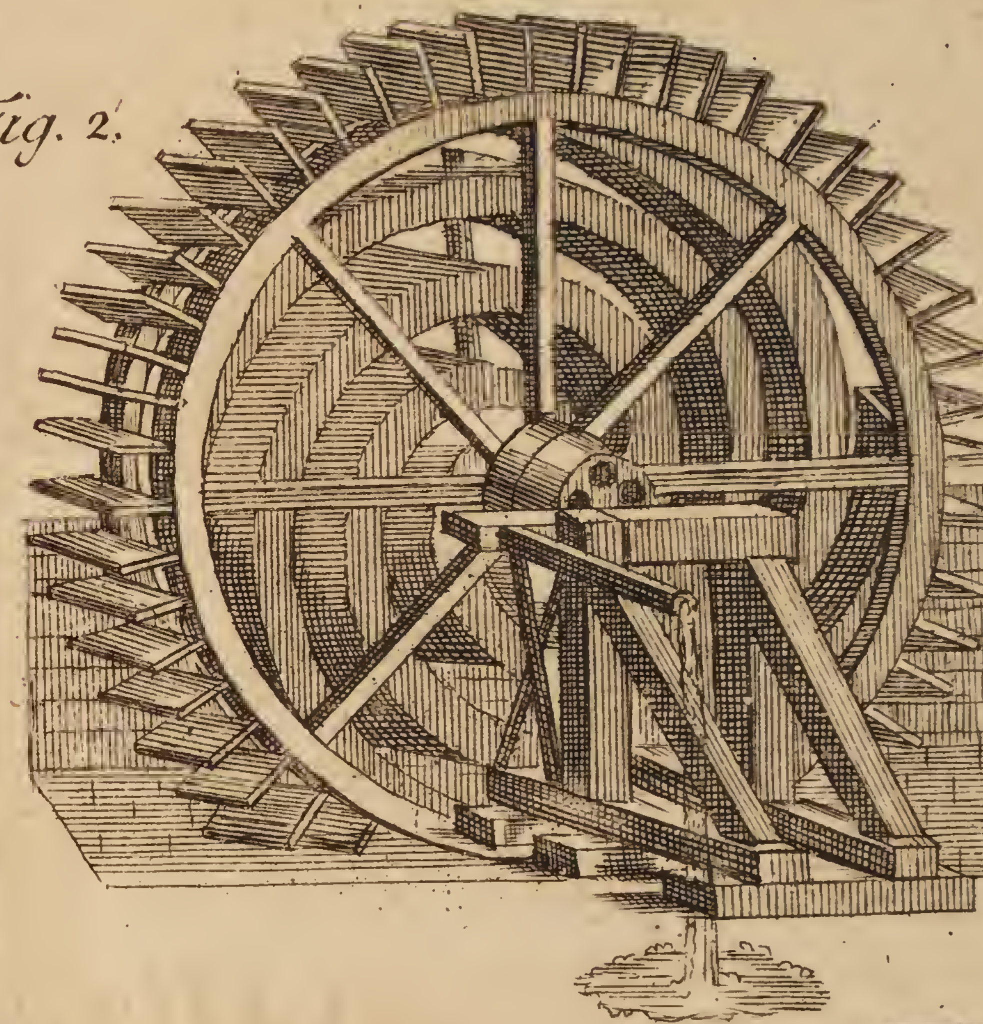


Fig. 2.

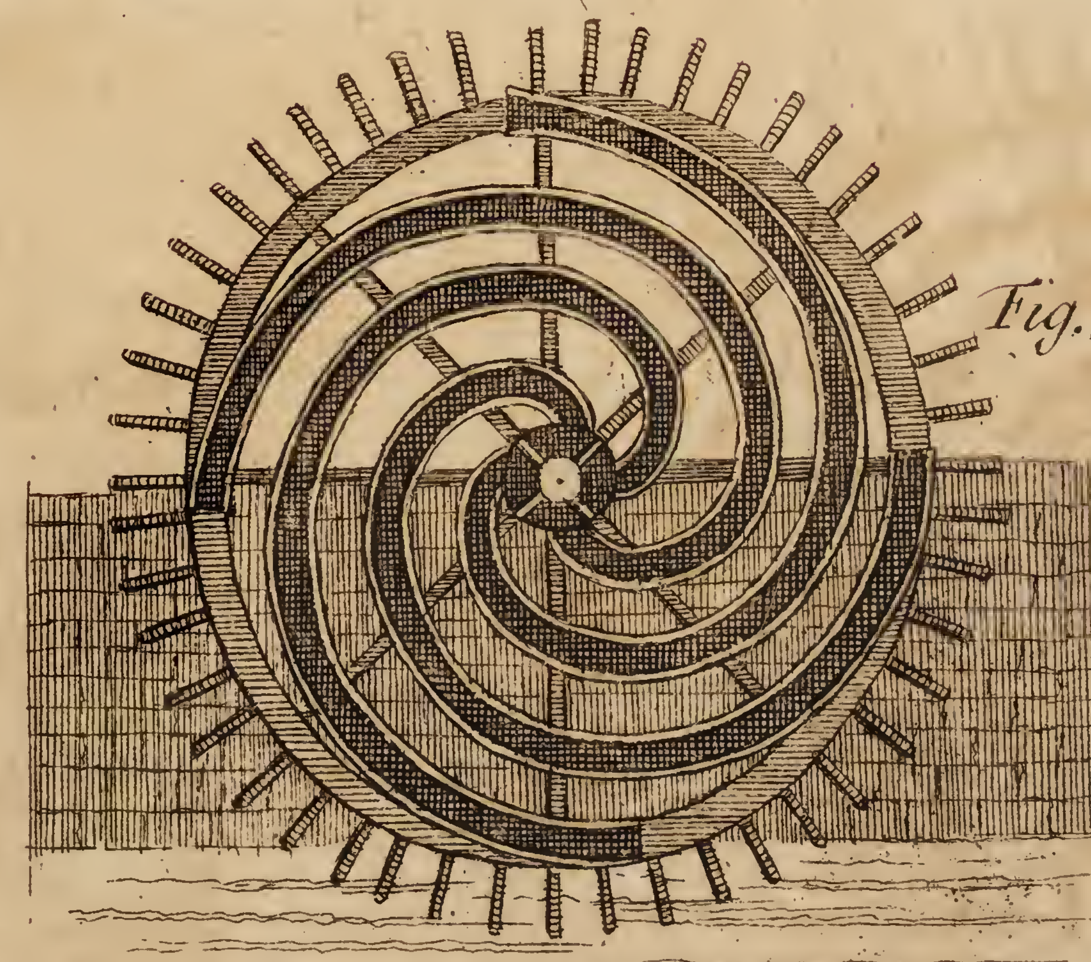


Fig. 3.

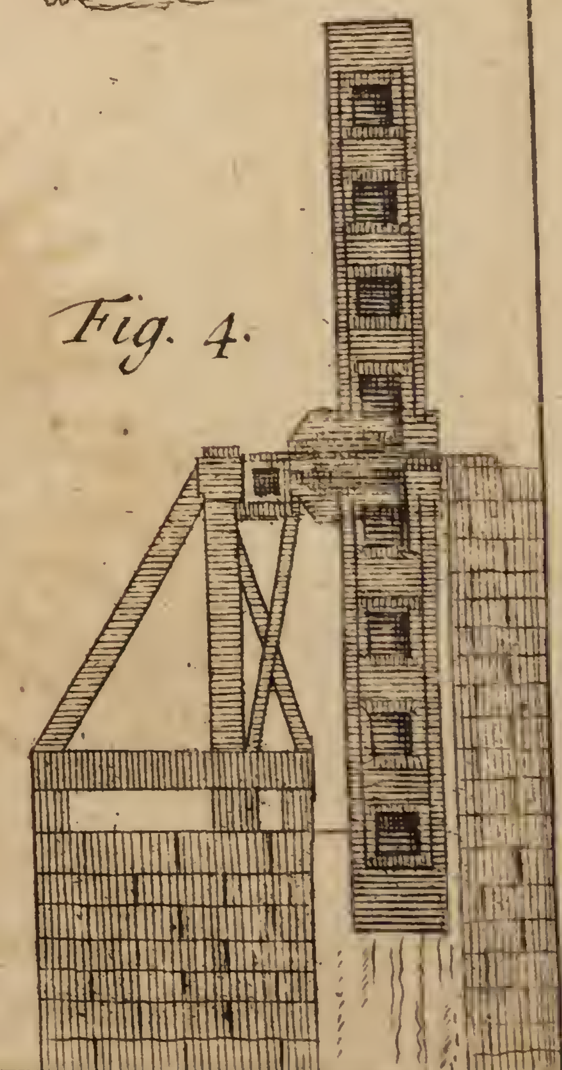


Fig. 4.

A. *One of the Tubes.* B. *The same laid open.*
C. *The inside viewed thro a Microscope.*

Fig. 2 the Machine in perspective. 3 & 4 two different sections of ye same Machine Fig. 5 one of ye Channels

J. Mynde sc.

the learned, if I informed them in this memoir of what I have been able to diſcover, with the greateſt certainty, both by the dates of the impreſſions of what has been publiſhed; as from the particular information that I have had in the intimacy which I have always entertained with thoſe who had the greateſt ſhare in them, and with our oldeſt mathematicians, for a great number of years that I have applied myſelf to theſe inquiries with relation to geometry and natural philoſophy.

I begin then with the micrometer, and I find that in the *ephemerides* of the marquis *Malvaſia*, printed in 1662, pag. 193, ſpeaking of *Saturn* and his ſatellite, which is the middle one of the five that accompany this planet; he names ſome perſons who had already ſeen it, and he does not mention M. *Huygens*, who from the year 1659, had printed his ſyſtem and obſervations upon the ring and ſatellite of this planet, which this marquis could not be ignorant of, and which he ought not to have paſſed over in ſilence. Since M. *Huygens* had publiſhed it three years, and had dedicated his book to prince *Leopold* of *Tuſcany*.

It is on this occaſion that the marquis *Malvaſia* relates, in page 196, the manner of obſerving ſmall diſtances between ſtars and planets, and even the means of preparing an exact figure of the ſpots in the moon. He makes a frame or little net with very fine ſilver threads, and divides again one of the ſquares of the extremity of this net into ſmaller parts with the ſame threads, and having applied this net to the common *focus* of two convex glaſſes of a teleſcope, he makes one of the ſtars that are near the equator come upon one of the threads, by turning the net or the teleſcope, as much as is neceſſary to make it agree with it; and he reckons by his pendulum of ſeconds,

conds, how much time is paſſed between the paſſage of the ſtar from one thread to another, of thoſe that are perpendicular to that upon which the ſtar moves; which by this means gives him the knowledge of the quantity of minutes and ſeconds of a degree that the intervals of the threads of the net contain, with regard to the length of the *focus* of the teleſcope.

We ſee therefore by this, that the marquis *Malvaſia* had a ſort of micrometer which was not very different from that which Meſſ. *Auzout* and *Picard* publiſhed in 1666, except in the manner of dividing this, and of rendering it very exact and convenient, by applying to it the threads of ſilk, which are very fine in proportion to thoſe of ſilver, and making uſe of a runner, which moves by a ſcrew for meaſuring the diſtances exactly. This marquis had alſo then a pendulum clock which marked ſeconds, and, as he ſaid, had been found at *Florence* ſome years before.

But as to the micrometer, we find toward the end of M. *Huygens*'s book of the ſyſtem of *Saturn*, printed in 1659, that is to ſay, three years before the printing of the marquis *Malvaſia*'s ephemerides, the manner of obſerving the diameters of planets by making uſe of a teleſcope, and putting, as he ſays, to the *focus* of the convex eye-glaſs, which is alſo the *focus* of the object-glaſs, an object which he calls *virgula*, of a proper thickneſs to contain the object that he would meaſure; for he gives notice, that in this part of the teleſcope, with two convex glaſſes, the ſmalleſt objects are ſeen very diſtinctly; and it was by this means that he meaſured the diameters of the planets, as he relates them after having found, by the experiment of the paſſage of a ſtar behind this body, how many ſeconds of a degree it contained.

tained. There is ſo little difference between the conſtruction of the micrometer that M. *Huygens* made uſe of, and that of the marquis *Malvaſia*, which appeared but three years after, that this cannot paſs for a diſcovery. Thus we muſt allow that we are indebted to M. *Huygens* for the invention of the micrometer, which they have ſince improved to the point it is now at.

As to the pendulum clock, if the marquis *Malvaſia* has ſaid, in 1662, that he had a pendulum clock, and that he made uſe of it, as he obſerves, it is a date that we may refer to that time; but not what he adds, that it was found at *Florence* ſome years before, no more than that which is printed in 1666 in the *ſaggi* of *Florence*, where it is ſaid that *Galileo* had thought of applying the pendulum to a clock, but that it was not executed till 1649 by his ſon, without obſerving how this application was made: but if this pendulum clock was in uſe from 1649, it is not probable that M. *Huygens*, who was intimate with all the learned men in *Europe*, and was very well known at *Florence*, ſhould have the aſſurance to print the conſtruction of this ſame pendulum clock, by *Adrian Ulacq* at the *Hague* in 1658, as a new thing, nine years after it had been executed at *Florence*, without being afraid of paſſing for a plagiary, and of producing for a novelty what was already very well known; for there is but one way of applying the pendulum to the clock, which is by ſubſtituting it for the balance of the common clocks, to rectify the motion of this balance, which is always very unequal.

In this application of the pendulum to the clock, they had not yet thought of rectifying the proper motion of the pendulum, which had been found alſo at *Florence* to be very unequal, according to the

the different extents of its vibrations, which M. *Huygens* afterward found, and printed at *Paris* in 1673 in his treatiſe, entitled *Horologium Oſcillatorium* which is one of the fineſt works that has been lately made upon geometry.

This invention of pendulum clocks ingages me to ſay ſomething of the portable clocks and watches, of which they rectify the motions of the balance, which is very unequal in itſelf by means of a little ſpiral ſpring that governs the inequality of the balance, which is ſo much in uſe, that they now make all their watches in this manner ; and I can affirm this becauſe it is an affair that paſſes entirely under my inſpection. This invention was propoſed at *Paris*, only by word of mouth, about 40 years ago, by M. *l'Abbé de Hautefeuille d'Orleans* very fruitful in mechanical inventions. M. *Huygens* who was then at *Paris* and who ſeems to have ſome right over rectified clocks, preſently made, as he ſaid, ſome experiments with his ſpring fire-tongs, and having obſerved that the vibrations or motions of the legs were pretty equal, he conſtructed a watch with a ſpiral ſpring, upon the principle of the equal motion of the vibrations of a ſpring, and preſented it to M. *Colbert*. The invention was found to be very beautiful, and ſeemed to be very uſeful, for it was viſible that the motion of the balance was very equal; but as M. *Huygens* was very much eſteemed and in favour at court, he had a mind to aſk the privilege of this ſort of watches, which he obtained very eaſily. But this was not enough, it was neceſſary in order to make a greater advantage of this privilege, and to draw ſome profit from it, which he had no need of, having a conſiderable penſion from the king, to get it paſſed in parliament. The *Abbé* who knew what paſſed, and took pains to obtain the

the right of his invention, did ſo much by his reaſons and his proofs that he hindered the paſſing of the privilege. Some of the moſt celebrated workmen who foreſaw well the injury that this would do them, engaged themſelves in it; the affair reſted there, and M. *Huygens* ſpoke no more of it; and they have always continued to make all watches with ſpiral ſprings.

I ſhould have finiſhed my memoir after this hiſtory, if it was not that we have yet in the practice of aſtronomy an inſtrument which is not leſs uſeful than the preceeding; it is the quadrant and its portions which we make uſe of to obſerve the height of the ſtars, and their diſtance between themſelves, and which carries teleſcopes inſtead of common ſights; and I do not find any certain epoch when they began to make uſe of it. Theſe ſorts of teleſcopic ſights have a very great advantage above the common or ancient ones, as well for aſtronomy as geography, in this that all ſorts of eyes may make uſe of them equally, and that as the teleſcope inlarges the objects, we not only ſee them greater but much more diſtinct, and we may make obſervations with them with great exactneſs. I am only ſurpriſed that the uſe of theſe teleſcopic ſights did not quickly follow that of the micrometer, for it ſeems to me that only this inſtrument was applied to the quadrants.

It was publiſhed here that this was M. *Picard's* invention; and it was not without foundation, for this reaſon, I aſked him one day what it was, he anſwered very cooly that M. *Auzout* had a great ſhare in it, and I have not been able to find exactly the time when they were applied to the inſtruments. I only ſee that in the book of the *meaſure of the earth*, which was made by M. *Picard*, and was printed in 1671, but begun in 1669, where

where it is ſaid, in page 3, *that it had been thought of for ſome years to put teleſcopes inſtead of the old ſights*; and it was theſe ſort of ſights that were then uſed. I thought to have found ſomething concerning theſe ſights in the *philoſophical tranſactions*, but I do not find any mention of them there. It is only ſaid, that in *Dec.* 1665, M. *Auzout* aſked Mr. *Hook* to communicate his method, by which he makes a *lens* wrought according to a ſphere, of which the diameter is 20 or 40 feet, ſerve to a teleſcope of 100 feet; and that in return he will diſcover another, by the means of which they may meaſure upon the ground with the teleſcope: and what I have, ſays he, propoſed to ſome perſons as a paradox, which is to meaſure the diſtances of places from one ſingle ſtation, without making uſe of any mathematical inſtrument; but I think that this might be underſtood by imploying only the micrometer which M. *Auzout* uſed, or a quadrant with teleſcopes inſtead of common ſights.

I know that there have been ſome celebrated aſtronomers and great obſervers, who never would make uſe of theſe ſorts of ſights, although they were in uſe in their time, becauſe at the beginning they had obſerved only with common ſights, for they ſaid they might be reproached with their firſt obſervations, not having all the exactneſs poſſible, ſince they had changed their method, which they would not have done, if the firſt had been certain. This reaſon does not ſeem to me allowable, when one only aims at the perfection of a work. Others have had much more reaſon to make ſome difficulty of uſing theſe new ſights, for they ſaid that in reality one of theſe ſights was very viſible and fine, being only a ſilk thread, which would be hardly viſible, if the eye-glaſs of

of the telescope did not make it perceived: but as for the other sight, which was the centre of the object-glass, it was invisible: but we have answered to this objection, by demonstrating that we are not less assured of the position of this invisible sight, than of the other.

In short, we may say in general, that the most curious and useful parts of the sciences and arts, either liberal or mechanick, were not at first found in the perfection that we see them at present, and that a slight idea that shall be published, and even pretty often, by ignorant people, and as it were by chance, those who have a deeper knowledge of geometry, and especially mechanicks, have improved by it, and afterwards carried it, as it were, by degrees, to the greatest point of perfection to which it seems possible to be carried. But to whom shall we ascribe the discovery of these inventions? I could relate several examples of them, and those very considerable; but I shall content myself with one alone, which comes quite to my purpose, the invention of telescopes.

The son of a *Dutch* artisan who made spectacles, held in one hand a convex glass, like those which the *presbytæ*, or old men, use; and in the other hand a concave glass, which is used for those that are short-sighted; and having by chance put the concave glass near his eye, and removing a little the convex glass which he held before it, he perceived that he saw through these two glasses some distant objects, much greater and more distinctly than he saw them before with the naked eye. He shewed this effect to his father, who soon after put together some of the same into little tubes of five or six inches long; and this was the first discovery of perspective glasses. This invention spread at the same time every where; and this

might be in 1609, for *Galileo* publiſhed his obſervations with teleſcopes in 1610, and he ſays that he had been informed of this diſcovery nine months, as may be ſeen in his *Nuncius Sidereus*. But *Galileo*, who was a good philoſopher, and curious in diſcovering the effects of nature, ſtopped there; and it is ſurprizing how with a teleſcope that he had made of the ſame conſtruction with the firſt *Dutch* ones, he was able to diſcover the motion of the ſatellites of *Jupiter*, for this teleſcope was about five feet long; and the longer they are, the ſmaller is the ſpace which they ſhew.

However, *Kepler*, a good mathematician, would penetrate further, and ſearch the cauſe of the effects of this invention, which he did in a very ſhort time; for he compoſed his *treatiſe of dioptricks*, and printed it in 1611, a year after *Galileo*'s *Nuncius Sidereus*. This work of *Kepler* is very fine and curious; and I am ſurprized that he was able to compoſe it in ſo ſhort a time, being then imployed in conſtructing his *Rudolphine tables*; and there is great probability that he had not thought of it before 1610.

M. *Deſcartes* came afterwards, and printed his *dioptricks* in 1637, which is a very fine work; wherein he carries his inquiries very far, and his demonſtrations upon viſion, and on the figure which lens's ought to have in order to compoſe teleſcopes; and at laſt he endeavours to conſtruct a very great teleſcope with a convex object-glaſs, and a concave eye-glaſs, which he could not make any uſe of, becauſe he could only ſee an almoſt inſenſible ſpace of the object. M. *Deſcartes* did not think of the advantage that he might draw from the combination of one convex glaſs for the object-glaſs, and another for the eye-glaſs, which his figures plainly ſhewed him; and without that

neither

neither the great nor ſmall teleſcopes would have been of any uſe for making diſcoveries in the heavens, and for the obſervation of angles; and how could M. *Deſcartes* be ignorant of what *Kepler* had remarked in the 86 propoſition of his *dioptricks*, where he ſays, ſpeaking of the combination of lens's or lenticular glaſſes, *Duobus convexis majora & diſtincta præſtare viſibilia, ſed everſo ſitu*; but he was not guilty of ſpending his time in reading the works of others, and was enough employed with his own ideas and experiments. It is therefore in 1611, which is the date of *Kepler*'s *dioptricks*, that we muſt fix the epoch of the teleſcopes with two convex glaſſes, and not at that of the book intitled, *Oculus Eliæ & Enoch*, by *F. Reita* a *German* capuchin, which did not come till a good while after. It is however true, that this father, after having ſpoken of teleſcopes with two convex glaſſes, puts before this teleſcope another leſſer teleſcope, compoſed alſo of two convex glaſſes, which reverſe the reverſing of the firſt, and makes the objects appear in their natural poſition, which is very convenient, but of very little uſe for the ſtars, in compariſon of the clearneſs and diſtinction which appears much greater with two glaſſes than with four, becauſe of the thickneſs of four glaſſes and eight ſurfaces, which have always too many inequalities and faults.

Nevertheleſs I think we have been a great while without making uſe of the teleſcopes with two convex glaſſes; and I do not believe that this was before the invention of micrometers, where we have ſeen that they are uſeful becauſe of the common *focus* of theſe two glaſſes, where the ſmalleſt object appears very diſtinctly.

I

I do not at all depend upon what *J. B. Porta* ſays in his *natural magick*, where ſome have believed that he had found out the invention of teleſcopes, which *Kepler* obſerves in his *dioptricks*, but he does not ſeem to be of that opinion; and beſides there is great probability that this would have been very publick, for *Porta* ſays that he had communicated his invention to ſeveral of his friends, who had their ſights too weak or ſhort, and found benefit by it, for his own words ſeem to prove the contrary; *Si utrumque*, ſays he, in ſpeaking of convex and concave glaſſes, *recte componere noveris & longinqua & proxima clara videbis*. Now it is certain that teleſcopes do not make us ſee diſtinctly the objects that are near us, as the reading of a book, but I believe rather he means only that we ſhould apply the convex glaſs againſt the concave, to take from one the too great convexity or concavity, with regard to the nature of the eyes, which ſtand in need of it. And beſides this book had been printed by *Plantin* in 1561, and reprinted at *Naples* in 1580; and it is not very probable that an invention ſo uſeful and ſo well known as that of teleſcopes might be, would have been neglected and buried in oblivion for near 50 years, till the diſcovery was made of them in *Holland*.

V. *Obſervations on the ſhell fiſh called* pinna marina, *or mother of pearl, on occaſion of which the formation of pearls is explained; by M.* de Reaumur *. *Tranſlated by Mr.* Chambers.

In a former memoir I have deſcribed ſome of thoſe artful means whereby ſhell fiſhes, and other

* Nov. 24, 1717.

ſea

ſea animals, are taught by nature to fix themſelves againſt the waves, and prevent their being toſt at random thereby; and I inſiſted particularly on muſcles, which are fixed at anchor by a conſiderable number of threads, which may be conſidered as ſo many cables, the method of ſpinning which was laid down at large. Theſe ſpinſters of threads, uſeleſs as to our purpoſe, I only conſidered as ſea caterpillars, but obſerved at the ſame time, that if the earth have its ſilk-worms, the ſea has its ſilk-fiſhes; and that the ſhell fiſh called by naturaliſts *pinna marina*, and on the coaſts of *Italy* and *Provence* mother of pearl, fixes itſelf like the muſcle by a kind of ſilken threads, which were manufactured by the antients, and are employed in certain works to this day. I added, that it was probable theſe threads were ſpun like thoſe of muſcles, but that I could not aſſert it, as not having been within the reach of the ſeas where this animal is found: and though I have continued at the ſame diſtance from thoſe ſeas, yet I have lately had an opportunity to obſerve the *pinna marina* by means of his highneſs the duke of *Orleans*, whoſe protection of the ſciences brings us all objects to our hands. The *pinna marina* being fiſhed near *Toulon*, that prince was pleaſed to ſend a paper, wherein we requeſted ſome of theſe fiſhes, and noted the precautions we would have them ſent withal, to M. *Hocquart* intendant of *Toulon*, which he performed according, ſending ſome in brandy and others in water, impregnated with as much ſalt as it would bear.

The *pinna marina* may be conſidered as a kind of ſea muſcle, only much larger than any of the reſt; ſome of thoſe brought us from *Toulon* being two feet and ſeveral inches long, their ſhell like that of other muſcles, conſiſts of two ſimilar and equal

equal pieces *, which from the *vertex* † of the ſhell widen inſenſibly till within a third of their length ||; from whence they begin to contract, but more abruptly, ſo as to form a curvity reſembling a ſemi-oval, whoſe leſſer axis is in the place where the greateſt breadth was fixed, this greateſt breadth being about two fifths of its length; they are much flatter than the other muſcles proportionably to their ſize, the largeſt not meaſuring above four inches in the thickeſt part, and being at leaſt half an inch thick at the *vertex* **, where they are about the ſame breadth as thickneſs, and do not form ſo ſenſible a heel as the other muſcles; all we perceive is a little convexity on one ſide ††; and the little concavity |||| on the other; but theſe ſides grow ſtraiter as they approach the *vertex*.

In moſt of the *pinnæ marinæ* I have found the ſpringy ligament, which keeps the two pieces together on the concave ſide; it ariſes in the *vertex* of the ſhell, and proceeds almoſt to the place where it ceaſes to ſpread; the two pieces are not bound together on the other ſide, but are bordered with ſeveral layers of horny matter, like that whereof the ſpringy ligament is formed; whereas the ſhells of other muſcles, on the contrary, are faſtened together on the ſide of the heel, or convex ſide; and I have even known ſome *pinna marina* which opened on the ſame ſide, and had their ligament on the other. It is ſomewhat extraordinary to find ſuch a variety in ſhell fiſhes of the ſame ſpecies; but what there is in common to them all, is that the edges of the

* Plate V. Fig. 1 and 4. † Fig. 1. A. Fig. 4. N. N.
|| Fig. 1. E. D. ** Fig. 1. A. †† Fig. 1. C. H. D.
|||| E. B.

ſhell

ſhell are thicker on the ſide where it opens, than on that where it is faſtened.

We have this further remarkable in the ſhells of the *pinna marina*, that upon viewing the ſurface of either piece, which was touched by the animal, there appears a band or *faſcia* of a ſubſtance like that of the ligament†, which ariſes in the *vertex*, and proceeds about half way of the length of each piece, dividing the breadth thereof into two parts, whereof that on the convex ſide, or which the ſhell opens at, is ſomewhat narrower than the other. This matter in ſome places penetrates pretty deep into the thickneſs of the ſhell. It looks as if there had been a fracture in each piece which had been filled up by this ſubſtance, and that the parts ſeparated by ſuch fracture had been but clumſily applied one to the other; for on the inſide they make an obtuſe angle, and want the roundneſs obſervable on their outſide. This kind of fracture is likewiſe found on the external ſurface of ſome ſhells, and may be ſeen in AG. *fig.* 1 As all ſhells have it at leaſt on their inner ſurface, there is no room to ſuppoſe that they had been really broke in this place; but it is more natural to conclude that this *faſcia* of matter, different from the reſt, marks the limits of a part of the body of the animal, which yields a juice like that wherewith the extremities of the ſhells are lined, while the other parts yield a juice proper to thicken and extend the ſhell.

But what is moſt remarkable in a ſhell of this fiſh is the difference of matter between the two *ſtrata* whereof it is compoſed. Part of the inſide* is of the colour of mother of pearl, extending from the *vertex* to upwards of half its length, and growing thinner and thinner as it approaches that limit;

† Fig. 1. R.S. * Fig. 4 VQQQ.

limit; ending at laſt no thicker than paper. The other layer or *ſtratum* ſerves only as a cruſt to this, and forms the whole thickneſs of the ſhell, where the former is wanting†. It is rough on the outſide, and its colour darkened by the mud adhereing to it, but on the inſide it is ſmooth, and of a pale red colour, which is the leſs ſenſible by reaſon of its thinneſs and tranſparency. The texture of this layer is ſomething ſingular, being formed of a multitude of threads laid cloſe to each other, whoſe length is the thickneſs of the layer, and conſequently their direction almoſt perpendicular to the plain of the ſhell. Theſe threads are ſlender, and yet may be diſcerned with the naked eye, but with the microſcope they are diſcovered to be ſo many little rectangular parallelipipeds with baſes nearly ſquare; and what ſurpriſes moſt is that in ſome parts of the ſhell they ſcarce adhere together, ſo that pulling off a piece of the cruſt, it covers the mother of pearl towards the *vertex*‖, which may be eaſily done, and rubbing it between the fingers the threads crumble and ſeparate from each other. But in making this experiment, it may be proper not to ſpread theſe ends of threads too much upon the hand, for want of which precaution I have ſometimes occaſioned very troubleſome itchings between my fingers, and on the palm of my hand, theſe threads being almoſt as fine as thoſe covering the pods of a kind of peas, vulgarly called ſcratch peas, or louſy peas, excite the like itchings as they do, but ſomewhat leſs pungent, by reaſon being a little coarſer they do not pierce the ſkin ſo deep.

If a piece of the ſame layer be taken towards the other end of the ſhell, the threads it is compoſed of, though equally viſible with the former,

† Fig. 4, TTT. ‖ Fig, 1. KK.

do

do not ſeparate ſo eaſily, for the layers next the *vertex* are the oldeſt, and as they grow old, they putrify as it were, and hence it is that the parts they are compoſed of tear aſunder by a ſlender rubbing. Accordingly it is frequent to find the mother of pearl bare about this part, the cruſt it was covered with being mouldered away||.

That part of the ſhell which has the mother of pearl colour, conſiſts of thin leaves laid parallelly one over another, ſo as the thickneſs of the ſhell is formed by that of the leaves, they are eaſily ſeparated from each other, by only calcining them a moment; the ſtructure therefore of this part of the ſhell reſembles that of ſlate and other foliated ſtones, while that of the other part reſembles the ſtructure of *amianthos* and certain fibrous talcs, *&c.* Yet I have obſerved layers or leaves in the fibrous part much like what are found in the other, and this even after calcination, in which caſe each thread or little parallelipiped was found to conſiſt of ſeveral ſuch parallellipipeds placed end to end.

I have ſince examined whether this ſtructure of a *ſtratum*, of threads covering another of *nacre* colour might not be common to ſeveral other kinds of ſhells, and have found it in the true *nacre* ſhell in thoſe called mothers of pearls, and ſeveral other kinds of ſhell fiſhes, but by reaſon the threads there are ſlenderer and adhere cloſer together, it had not been eaſy to diſcover as much, unleſs the *pinna marina* had firſt pointed them out.

It is more eaſy to account for the formation of theſe ſtony threads, and their regular figure, and uniform arrangement, then for the formation of ſtones of a like ſtructure; here we have organs for the ſtony juice to be moulded in, and may conceive orifices of veſſels to let it drop from all of

|| Fig. 1. KK.

them, diſpoſed orderly, and each of a rectangular figure, which juice may be ſuppoſed to retain the figure it had here aſſumed, whereas that which forms the *nacre* being more fluid, does not retain the figure of the orifice it paſſed through, though it is not impoſſible but that theſe threads or little parallelipipeds might have been faſhioned ere the juice iſſued from the body of the animal, as will appear from an obſervation to be made hereafter.

Authors who ſpeak of this ſhell fiſh, inform us that its ſituation in the ſea is vertical, viz. with the point or *vertex* downwards; but it is from the fiſhermen doubtleſs they took the account, which it is not very eaſy either to prove or diſprove; leſs doubt is to be made as to the other point affirmed by the ſame fiſhermen, viz. that the *pinnæ* are always faſtened to rocks, or neighbouring ſtones by a bunch of threads*, which is always to be broke in order to get the animal up.

They fiſh for them at *Toulon* at fifteen, twenty, or thirty feet depth of water, with an inſtrument called a cramp†. It is a kind of iron fork, whoſe grains or forks a a are perpendicular to the ſhaft d d, and about eight inches long and ſix aſunder, in the wideſt part; the length of the ſhaft being proportioned to the depth they are to fiſh at. With this inſtrument they lay hold on the *pinnæ*, tear them looſe, and bring them up.

The lock of ſilk or thread proceeds immediately from the body of the animal, iſſuing from the ſhell at the ſide, whereby it opens four or five inches diſtance from the *vertex*, in thoſe of the larger ſize††.

This ſilk ſerves to detain the *pinna*, and prevent its driving with the water, but cannot hinder its being thrown down and overturned, nor keep it

* Fig. 1. IH. † Fig. 3. †† Fig 1. H.

in the ſuppoſed vertical ſituation, ſo that it is probable the ſhell is ſometimes inclined to the horizon, and ſometimes lies flat like the muſcles and other ſhell fiſhes, which do not bury themſelves in the mud. It cannot be warranted that they have ſuch threads all along them, but I have known ſome which extended ſeven or eight inches, and weighed three ounces; theſe threads are of a brown colour and extremely fine, being uſually ſo entangled that it is not eaſy to get their whole length.

The mechaniſm whereby theſe threads are formed, was the chief object of my enquiry, my firſt view being to aſcertain whether they are ſpun by the *pinna marina* as muſcles ſpin theirs. This point I have no room now to doubt of, if we may conclude that ſimilar parts placed after the ſame manner ſerve in different animals for the ſame uſe. There is a part in the preſent fiſh, like what we have called the wier drawing inſtrument in the muſcle, and ſituate in the ſame place*. All the difference between the parts of the two animals being only what the difference of their effects require. The *pinnæ marinæ* need much longer and ſlenderer threads, and their ſpinning inſtruments are longer and ſlenderer. Accordingly in ſpeaking of that of the muſcles, we obſerved that when the animal is dead, or in a ſtate of inaction, it is not above half an inch long, whereas in the time of action it extends to upwards of two inches long, conſequently that of the *pinnæ marinæ*, which when the animal is dead is ſometimes upwards of two inches long, is only extended at the ſame rate, as that of the muſcles, when it meaſures ſix or ſeven inches which is the length of its longeſt threads; for the ſpinning inſtrument here muſt always be the ſame length as the thread ſpun, the caſe being quite al-

* Fig. 4. Y.

tered

tered from the method of the wier-drawers, or even of caterpillars and silk worms, for that has been already observed, the present instrument is only a mould wherein a viscid juice takes the figure and consistence of a thread, and that this mould opens its whole length on one side to let the thread out it has formed. This cleft or aperture of the *pinna*, for the discharge of its thread is perfectly like that of the muscles, only narrower and less deep; in fine, the threads have their origin near that of the spinning instrument, and are lodged in a kind of membranous bag of a conical figure†, in the *pinna*, as well as in the muscles.

But nature who makes no resemblance so perfect as not to mix some diversity therewith, has given the *pinna marina* certain parts which are peculiar to them. In this membranous bag, from whence the threads proceed, there are five fleshy leaves*, shaped somewhat like semi-ovals, and having their length in the same direction as that of the shell, between these are four others of the like figure, but much smaller ††, which seem of a cartilaginous nature, but when closely examined are found a kind of *plexus* of threads not much interwoven, but so well applied against each other, as to form a smooth continuous body. About the middle of the leaf is a place thicker than the rest, which appears wavy or curled as it were, being formed of threads bent as in ** r s, and closer to each other than elsewhere; these latter or silky leaves separate the former fleshy ones, and from them proceed all the threads which fasten the *pinna marina*, and form its plume or tuft, or rather the plume consists of the same threads as the leaves only prolonged, and parted from each other; some of them quit

† Fig. 4. Z.
†† Fig. 6. n n, o o.
* Fig. 5. ll, k k, L.
** Fig. 7.

the leaf on various parts of its edge, but the generality attend it to the end next the riſe of the ſpinning inſtrument. Moſt of theſe laſt ariſe from that part of the leaf already obſerved to be thicker than the reſt, the threads proceeding from all the four leaves meet towards the origin of the ſpinning inſtrument, as much as ſuffices to form a packet or begin a plume*. The *pinna marina* has ſuch a prodigous ſtock of threads, that it could not well have contained a trunk big enough for them all to be faſtened to, whence they are more commodiouſly faſtened to theſe flat leaves. The fleſhy leaves which ſeparate the ſilky ones, have probably other uſes, and may ſerve to do the office of lips, to apply and faſten the end of a new formed thread to a leaf.

The other internal parts of the *pinna marina*, are much like thoſe of the muſcle, they are faſtened like them to their ſhells by two ſtrong muſcles, one of them near the *vertex*†, and the other about the middle of its length towards where the *nacre* ends **, for it is only the part thereabout, between theſe two muſcles that produces it. Near the ſecond, or largeſt of theſe muſcles, is the *anus* X ſituate, and the mouth†† near the firſt, being cloſed by a ſemi-oval lip, which the ſea muſcle has nothing of. But to enter into a larger detail of the internal parts of this ſhell-fiſh would require freſh ones, and that in large numbers to ſupply reiterated diſſections, which it were to be wiſhed ſome able anatomiſts would take in hand, this being the largeſt of all the two leaved or bivalvous ſhell fiſhes in our ſeas, would be moſt commodious for diſſection, and perhaps the fitteſt to give an inſight into the ſtructure of the animals of this claſs;

* Fig. 6.
** Fig. 4.
† Fig. 4. V.
†† Fig. 8. u.

beſides

befides that, it appears the moft proper to let us into the myftery of the formation of pearls. They produce ftore hereof *, which are of different colours, reddifh, blackifh, yellowifh, and tranfparent as amber, all of them indeed fhort of the water of the *Indian* pearls, though the livid or lead coloured ones come the neareft. The publick has fixed no value upon thefe pearls, and yet to a naturalift they are infinitely more valuable than the pearls of any oriental oyfters, as exhibiting fome particularities which relate to the formation of pearls in general, not to be met withal in the others.

I fhall not relate all the fables the antients have left us upon the origin of pearls; phyfiology is too far advanced to need a proof that they are not produced by the dew of heaven, notwithftanding what fo many grave authors have pofitively afferted. Nor does the opinion of thofe who take them for the eggs of the animals they are found in, deferve much more attention. M. *Geoffroy* the younger, ranges them among bezoards, as putting under that clafs all ftones formed of *ftrata* generated in animals. Withal we may affirm that they are only to be regarded as the other ftones formed in animals, *viz* in the bladder, kidneys, *&c.* and are evidently like them, the effect of a difeafe in the fifh: 'tis no wonder that an animal which has a ftony juice circulating in its veffels fufficient to build, thicken, and extend a fhell, fhould have enough to form fuch ftones in cafe the juice deftined for the growth of the fhell happen to be extravafated, and be poured into any cavity of the body, or between its membranes.

This ftone is called a pearl when the extravafated juice it is formed of proves of a filver co-

* Fig. 4. z z z z, y y.

lour

lour or water, approaching that of *nacre*; and ſuch muſt its colour be in oyſters, muſcles, and other fiſhes, whoſe ſhell itſelf is the colour of *nacre*; the beauty of the water of the pearl may even ſurpaſs that of the *nacre* of the ſhell, though both formed of the ſame matter; that of the one being carried to the outſide of the body of the animal, where it is frequently in contact with muddy waters, which alter its colour; whereas the matter of the other, being received within membranes, is protected thereby.

Accordingly I have always been of opinion, that no other matter need be ſought for the formation of pearls, than that which forms the *nacre* of the ſhell, but I have now ſomething more than probability for my opinion, the *pinna marina* having furniſhed me with ſome obſervations which give it an air of certainty.—We have already obſerved, that they are found of different colours, but there are chiefly two, the one whoſe ſhades approach the *nacre* colour, and the other a reddiſh colour. It has been likewiſe obſerved, that the ſhell conſiſts of two *ſtrata* of different colours, the one reddiſh, and the other *nacre* coloured, which of itſelf indicates that there are ſtony juices in the ſhell, of the proper tinctures to furniſh the two principal kinds of pearls. If the veſſels, which convey the juice deſtined to form the *nacre* part, happen to burſt, they will form a pearl of a *nacre* colour. On the contrary, if thoſe veſſels burſt which contain the juice, whereof the other part of the ſhell is formed, the extravaſated juice will form a reddiſh or amber-water pearl, according to the colour of the ſhell it would have formed.

We have alſo obſerved, that a part of the ſhell from the *vertex* as far as the ſecond muſcle, has a

layer or *ſtratum* of *nacre*, which the reſt of the ſhell is without; whence it follows, according to what has already been ſhewn elſewhere of the origin and growth of ſhells, that thoſe parts of the body of the animal which are contiguous to the firſt part of the ſhell, furniſh a *nacre* coloured juice; and thoſe on the contrary, contiguous to the reſt, a reddiſh juice like the ſhell formed of it.

There are both pearls found in thoſe parts of the body of the *pinna marina*, which correſpond to the places where the ſhell is white, and in the parts correſponding to the places where it is reddiſh; but an obſervation I have made which ſeems to decide the difficulty, whether pearls be formed of the ſame juice with the ſhell, is that the pearls I have met withal in thoſe parts of the fiſh which form the ſhell of a reddiſh colour *, were reddiſh coloured themſelves, and thoſe found in the parts of the fiſh where the ſhell was of a *nacre* colour †, were *nacre* coloured themſelves.

There is no part of the animal where I have found more of them, than in that muſcular part indented like a cock's comb, which borders the end of the ſhell and part of its circumference ||. It is this part that extends the ſhell, and of conſequence forms the upper layer, which is always reddiſh; and accordingly the pearls I have found therein, have always been reddiſh. Thoſe pearls, on the contrary, which I have found in the parts near the origin of the ſpinning inſtrument ‡, were always of a *nacre* colour, by reaſon the veſſels which ſupply the *nacre* are hereabouts.

I would not however undertake that there are no reddiſh pearls formed about the parts fartheſt from the edges, or even *nacre* coloured pearls

* Fig. 4. zzzz. † yyy. || zzz. ‡ yz.

near

near the edges; the veſſels which convey the ſtony juice to the edges of the ſhell, have probably their origin further off, where they may chance to be burſt, and ſo the veſſels which furniſh the *nacre* colour juice may paſs near the edges, and perhaps make a number of circumvolutions thereon. This at leaſt is certain, that the veſſels of each kind are moſt numerous, largeſt, and fulleſt of juice, in thoſe places where they contribute to the growth of the ſhell, and of conſequence ruptures and other accidents tending to extravaſate their juice, ofteneſt happen here.

There are alſo blackiſh or duſky coloured pearls found in the *pinna marina*, but theſe are opaque, whereas the amber coloured ones are tranſparent. They are all however formed of the ſame matter, which in the black has been ſince darkened by the mixture of ſome other juice, the animal having juices proper for that end. But even the black pearls, when broke in very ſmall pieces to render them tranſparent, become likewiſe of a colour like that of yellow or reddiſh pearl, it being their thickneſs that gives them the greateſt part both of their opacity and dark colour. There are alſo places where the colour of the ſhell is duſkier and more opaque than elſewhere, approaching near to that of the pearls juſt mentioned.

Beſides the difference of colour obſerved in the two *ſtrata* of the ſhell, we have noted another more remarkable one, *viz.* A diverſity of texture, the *nacre*, conſiſting of *laminæ* or leaves laid one over another, and the reddiſh part of threads laid one againſt another, to determine finally whether the ſilvered pearls be formed of the ſame juice with the *nacre*, and the amber coloured ones of the ſame juice with the reddiſh part of the ſhell,

 all

all that ſeems required, is to diſcover whether the ſame diverſity of texture ſubſiſt between the different coloured pearls, as between the different coloured parts of the ſhell. Accordingly I have broke ſeveral of each kind, and have always found the ſilvered pearls formed of concentric *ſtrata*, inveſting each other like the ſkins of an onion *, which is an obſervation far from being new. At the ſame time I have obſerved, that the reddiſh pearls have likewiſe concentric *ſtrata*, though not ſo viſible as the former: but beſide this, they have alſo threads like thoſe of the reddiſh ſhell, which were directed like ſo many *radii* from the centre towards the circumference †; ſo that we have the ſame varieties of colour, and the ſame varieties of ſtructure between the two ſpecies of *ſtrata*, and the two ſpecies of pearls; can there any doubt then remain, whether the *ſtrata* of the ſhell and the pearls, which bear ſo near a reſemblance, were not formed of the ſame matter.

I have ſeen ſome of theſe pearls half whoſe ſurface was *nacre* coloured, and the other half blackiſh; theſe had been generated in the conflux of two differently coloured veſſels, but there was only a thin layer of each of the two colours, the reſt being of one uniform colour.

When the veſſels burſt on the external ſurface of the body of the animal, or by being too much diſtended let their ſtony juice ooze out, the matter coming to fix and harden, forms a kind of knob, called a wen of pearl when found in the ſhell of a mother of pearl. The like knobs are alſo found in the ſhells of the *pinna marina*, and theſe both of a reddiſh and a *nacre* colour: thoſe in the ſhells of the true mother of pearl are ſometimes ſo

* Fig. 10. † Fig. 12.

like

like pearls in their water, that there is no diſtinguiſhing the one from the other. Accordingly when the jewellers find any of theſe wens hemiſpherical, they ſaw them off, and cementing two of the ſame ſize together, compoſe one pearl.

Nothing is more variable than the number of pearls in theſe ſhell fiſhes, there being ſome *pinna marina* where I have found none, and yet have known upwards of twenty in others; nor are we aſſured that the *pinna marina* have the like number of pearls every where as they have on the coaſt of *Provence*. As the air and food of certain countries render the inhabitants ſubject to certain diſeaſes, ſo the waters of ſeas and rivers, which the fiſhes reſpire, and wherewith they are partly fed, do doubtleſs occaſion them ſeveral diſorders. Muſcles, of the ſame ſpecies, have pearls in ſome rivers, and none in others. Our oyſters would probably be much more valued, if the water of our ſeas were as unwholſome to them as that about the ſhores where the pearl fiſheries are to the oyſters that live in them: for whereas pearls are rare in our oyſters, they would then probably be common when thoſe ſhell fiſhes were more ſubject to the ſtone.

An explanation of the figures in plate V. *tranſlated by* J. M.

Fig. 1. The ſhell of a *pinna marina*, compoſed of its two pieces.

A the ſummit or *vertex* of the ſhell. BC the breadth at the *vertex*. The thickneſs is taken upon a line perpendicular to that which ſhould be drawn from B to C, and croſs the two pieces.

CHD the convex ſide. The line CD ſhews the place where the ſhell opens. Beyond CD is ſeen a little part of the ſecond piece of the ſhell.

BE

BE the concave ſide, where the hinge or ligament of the ſhell is, which goes to E.

F the extremity of the ſhell, where it opens.

H the place out of which the tuft proceeds.

I the tuft of ſilk.

GA marks the line which divides each piece of the ſhell in two.

The part EFDG is almoſt all of a reddiſh colour; and from G to A there is *nacre.*

KKK diſcover the *nacre*; the cruſt or reddiſh *ſtratum* compoſed of threads is detached, which happens naturally.

Fig. 2. A piece of the cruſt compoſed of threads drawn by a magnifying glaſs. L the threads drawn ſeparately. M a little tuft of like threads.

Fig. 3. The inſtrument with which they fiſh the *pinna*; a a its forks; b c the place where the iron is faſtened to the ſtaff; d d the ſtaff; e e the plane of the fork.

Fig. 4. A ſhell which has been opened by force. The animal remains almoſt entire upon the piece of the ſhell NOO; there is hardly any upon the part NPP. QQQ mark that part which is of the *nacre* colour.

R ſ is the band of matter approaching to that of the ſpring, which divides each piece in two.

TTT the part which is reddiſh, or of an amber colour.

Near N are ſeen ſeveral *ſtrata* one above another; they are *laminæ* of a matter like that of the ſpring, and mark the different places which the animal has ſucceſſively abandoned.

At V is the ligament near the *vertex.*

X is the mouth.

Y the ſpinning inſtrument.

Z the membranous bag, which covers the fleſhy *laminæ*, and the cartilaginous ones, from which the threads, that form the tuft, proceed.

At &c. is the *anus*; we ſee there a large muſcle, which faſtens the fiſh to the two pieces of the ſhell.

Fig. 5 and 6 repreſent each of them a part of the cavity Z of fig. 4. Here the ſides which contain it have been opened.

f g, f g, are the two ſides open.

b is the end of the ſpinning inſtrument cut at b.

l l, k k, L are five fleſhy or muſcular *laminæ* contained in this cavity; here the ſilky *laminæ* are removed, which ought to be between them.

Fig. 6. n n, o o, part of the four ſilky *laminæ*, which here are in place, and ſeparated from one another by the fleſhy *laminæ*; m m the ſides of the cavity; p the end of the ſpinning inſtrument.

Fig. 7. is a part of a ſilky *laminæ* taken toward the middle, and drawn by a magnifying glaſs; ſ r marks this place thicker than the reſt, where the threads run acroſs. At r theſe threads begin to ſeparate, in order to compoſe part of the tuft.

Fig. 8. ſhews the mouth of this fiſh more large than in Fig. 4. t the mouth; u the lip, which commonly covers it, and has been raiſed in this figure; x a pearl.

Fig. 9. is a pearl of a *nacre* colour.

Fig. 10. is the ſection of the ſame pearl, which ſhews the diſpoſition of the *ſtrata*.

Fig. 11. is one of the brown pearls of the *pinna*.

Fig. 12. is the ſame pearl broken, to ſhew the direction of the rays, from the centre toward the circumference.

In

In Fig. 4. zzzz are pearls of an amber or reddiſh colour; and yy pearls of a *nacre* colour.

VI. *Remarks upon the loadſtone; by M.* de la Hire *.

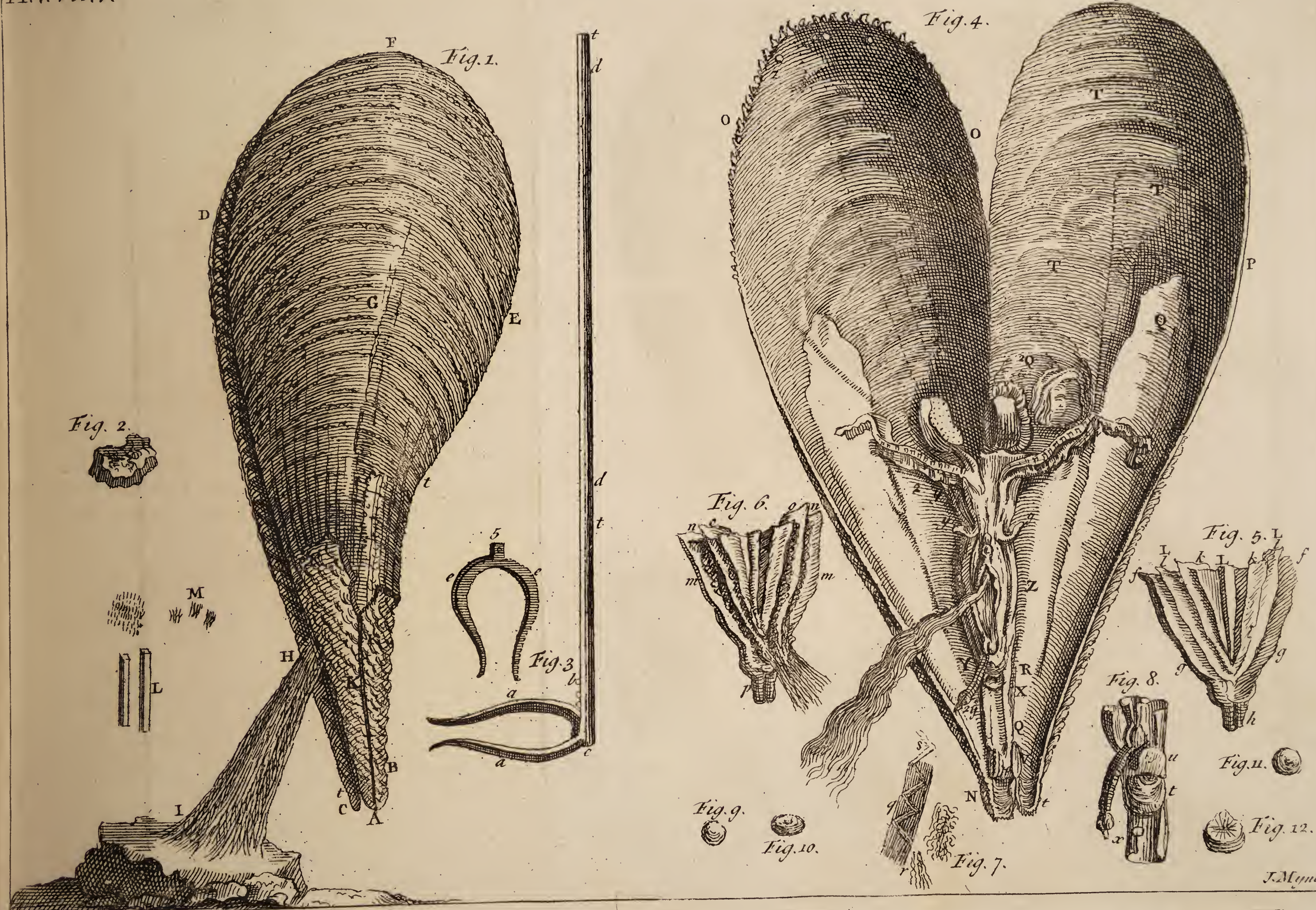
We can hardly be perſuaded that a loadſtone can take away from another loadſtone that is ſtronger than it, a bit of iron which it holds ſuſpended; for it ſeems that the virtue of the ſtrongeſt muſt always prevail over the weakeſt. But we cannot doubt of the experiment, for I have made it carefully, to ſee if what M. *Rohault* relates in his phyſicks was true. He calls this experiment *a fine difficulty*; and for the reſolving it has given no other reaſon, but *that the iron touches the weakeſt loadſtone in more parts than it touches the other.* It muſt be obſerved that this experiment does not always ſucceed, as M. *Rohault* alſo ſays; and it is without doubt that this has given him room to conjecture, that this only proceeds from the different touches between the iron and the loadſtone, which ſeems very probable; but we ſhall ſee in the end, if this reaſon may be generally maintained, and from whence theſe differences proceed.

I firſt obſerve, that to make the experiment in queſtion exact, we muſt not ſuſpend a bit of iron to a loadſtone, ſince the weight of the iron will always tend to ſeparate it from the upper loadſtone: for this reaſon it is better to place the loadſtone ſo that the line of its poles may be horizontal, and that the iron, which I ſuppoſe to be a little rod, which ſhall be applied to its ſtrongeſt pole, may be alſo horizontal in its length, and placed upon a poliſhed body, as of

* Dec. 11. 1717.

glaſs,

Pl. V. Vol. V. p. 272.



glaſs, that it may be able to ſlide caſily, as may be ſeen in the figure * where A is the great and ſtrongeſt loadſtone, of which M is the ſouthern pole, which is eſteemed the moſt vigorous in theſe countries, and againſt which the iron rod F is applied, the loadſtone B is the weakeſt; of which the northern pole ſ is applied to the other end of the rod F; and it is obſerved that when we draw away the loadſtone B from the loadſtone A, according to the direction of its poles, ſometimes this loadſtone B draws from the loadſtone A the iron rod F, and carries it along with it, remaining always applied to its pole ſ; and ſometimes the loadſtone B parts from the iron rod, which remains joined to the loadſtone A, as one would think ſhould always happen.

But the experiment uſually made to find the direction of the poles of a loadſtone, which is to ſprinkle very lightly ſome ſteel filings upon a paper placed above a loadſtone, and nearly according to its poles, has made me ſuſpect whether there were not in all loadſtones ſome pores, through which the magnetick matter diffuſed in the air, would more eaſily be introduced than in others; for we always obſerve, that the ſmall grains of this ſteel diſpoſe themſelves in threads ſeparated from one another, and never otherwiſe, if it be not out of the ſphere of the virtue of the ſtone, where this ſteel is ſprinkled indifferently, and without any regularity. Therefore in a loadſtone which ſeems to have but little force, it may however have pores which receive more magnetick matter, and conſequently may have a greater effect in experiments than many pores of a greater and ſtronger loadſtone: and if it was ſo, it would be eaſy to ſee why a weaker loadſtone in a cer-

* Plate VI. Fig. 1.

 tain

tain poſition with a little rod of iron, ſhould hold it, and take it away from another loadſtone that in general is ſtronger; and that in other poſitions the ſtrongeſt loadſtone would hold the iron when the weakeſt is taken away. But as what I have ſaid is only a conjecture, I had a mind to ſee whether the experiments would not give me ſome light into this affair.

For this purpoſe I took a great loadſtone which weighed about 6 *lb.* and is pretty ſtrong, ſince its ſphere of activity is ſenſible upon the needle of a compaſs at the diſtance of ſix feet; this loadſtone is quite naked and unarmed; it is a little irregular, except toward its ſouthern pole, which is terminated by three faces, one of which is much greater than the others; and it is this point which has always ſerved for touching of needles: and as I have always thought that a little loadſtone, which does not ſeem to have much force in comparing it to another which makes greater effects, may however be ſtronger in ſome one of its parts, I took a little bit of iron an inch long, and about three lines in thickneſs, and having touched it with the ſtone, I imagined that this bit of iron could never have as much virtue as the ſtone that had touched it; and as by the touching of it this iron became a ſort of loadſtone, I made uſe of it inſtead of the weak loadſtone B, to make the experiment by it with regard to the great loadſtone.

I therefore applied againſt the great loadſtone A*, a little iron rod or wire F, about a line thick, and an inch and half long; this wire was animated immediately, and held ſtrongly to the ſtone, and remained fixed there horizontally, according to the poſition of the poles of the ſtone. I afterwards applied to the other extremity of this wire

* Fig. 2.

wire F the bit of iron B, which had been firſt animated, ſo that the poles of contrary virtue touched one another; that is to ſay, the north pole ſ of the iron B touched the ſouthern pole of the wire F, that they might be more ſtrongly united together, and with the loadſtone A, the whole being placed on a level. After having drawn away ſoftly the iron B, the wire F remained fixed to it, and quitted the loadſtone A; and this always happened the ſame every time that I repeated this experiment; but it is true, that when I took away the iron B from the loadſtone A, about two or three inches, the wire F quitted alſo the iron B.

I had afterwards a mind to ſee what would happen, if I ſhould firſt apply the animated bit of iron B againſt the loadſtone, and then againſt this the wire F, and always the poles joined to the poles of different denominations; for then the wire would only touch the iron B in a little place, and the iron B would touch the loadſtone A in a much larger place, for the extremities of theſe irons had been well filed. And I have obſerved with ſome ſurprize, and againſt the inclination that I might have for M. *Rohault*'s reaſon, that the wire being drawn backward, carried away with it the iron B, which touched the loadſtone A, and always ſo in the repetition of the experiment.

I thought I ſhould not confine myſelf to theſe experiments alone, but make ſome others to endeavour to diſcover the true cauſe of the effect that we here ſpeak of; I therefore began to place my great loadſtone in ſuch a manner, that its large face and its poles were in an horizontal poſition; and having put a white paper upon it, I ſtrewed ſome ſteel filings in the common manner, and I

obſerved the poles and bellies as they are called, of the vortex of the magnetic matter which circulates about this ſtone.

Afterwards having taken away the paper, and placed the iron rod F againſt the loadſtone A, and at the end of this iron, the iron B, the length of both theſe irons being in the line of the poles of the loadſtone, as I had at firſt placed them, I put the paper again upon it, and having ſtrewed the filing, I obſerved, as may be ſeen in the figure*; that the threads of the magnetic matter, which came out through the pole M of the ſtone, ſeemed to lay along the rod F, without entering into it, and that from the iron B, which was applied to this rod, there proceeded threads almoſt perpendicular to the length of this iron, which meeting thoſe which came out of the pole M of the loadſtone, carried them away with them, and that at the extremity D of this iron, it formed a kind of pole or vortex very ſenſible which extended a great way, joining itſelf to the matter that came from the pole M, which might afterwards return toward the other pole of the loadſtone. What I ſay of the matter which proceeds from the poles of the ſtone, and from the irons muſt be underſtood in the ſame manner as that which ſhould endeavour to enter again, which is indifferent, and would cauſe the ſame effect according to M. *Huygens* ſyſtem. The ſame diſpoſition of the threads happen alſo if we place the iron B, near the loadſtone, and the iron F beyond it.

Theſe experiments would perſuade me that the animated iron or ſteel has more force than the loadſtone itſelf that animates it, and we often ſee that a rod or rule of a certain ſteel having been animated by a good loadſtone, ſuſtains or carries a much

* Fig. 2.

greater

greater weight, then the loadſtone itſelf naked and unarmed. I think that we might ſay that it proceeds from this, that ſteel or iron being a ſoft body in proportion to the loadſtone, although they are of the ſame nature, has its pores much more proper to receive the impreſſion of the magnetic matter, than the loadſtone has, which is a hard body, ſince it is a ſtone, the iron alſo loſes its virtue all at once to take another quite contrary, which a loadſtone cannot eaſily do, and which can only happen in ſome one of its parts, which ſhould not be intirely ſtone; beſides by degrees it again takes its primitive virtue, after it has been changed or altered by a ſtronger loadſtone, and conſequently it may introduce into the ſteel a much greater quantity of magnetic matter, than into the loadſtone itſelf which has directed and opened the pores of the ſteel to receive this matter.

Thus the two peices of iron D and F which touch one another, and one of which is applied againſt the load-ſtone A, may have a much greater virtue than the loadſtone A, and eſpecially when they are a little diſtant from this loadſtone, from which they may receive a part of the magnetic matter of its vortex, which joins itſelf to that which is diffuſed in the air. Alſo when theſe two irons are at rather too great a diſtance from the loadſtone A, to receive the magnetic matter, their force diminiſhes conſiderably, and it is what is commonly obſerved that a bit of animated iron, has much more force to ſuſtain a weight in preſence of a loadſtone than when it is at a diſtance from it. All this is viſibly known by the diſpoſition of threads of ſteel filings.

We might alſo bring for a proof of what I have before advanced, that a loadſtone may communicate to a piece of iron more force than it has itſelf

ſelf. For we do not queſtion but that the earth is a loadſtone, but a very weak one, becauſe the magnetical matter which ſurrounds it, is too much diſperſed about its globe, and that there are but few of its parts which can meet this iron, for this reaſon, at firſt it can only weakly animate a long rod of iron, that is diſpoſed in the air according to the courſe of this matter; yet it is this ſame matter, which in length of time, directs and opens all the pores of it, to make this iron become a very good loadſtone, after it has changed its nature by ruſt, and is become a ſtone, as we have ſeen at *Aix* in *Provence*, and at *Chartres*. It was this which engaged me twenty-five or thirty years ago, to encloſe in a ſtone of the ſame nature with that of the ſteeple of *Chartres*, ſeveral iron wires, according to the courſe of the magnetic matter, and which being at firſt animated, were converted into ſtone by ruſt, and became loadſtones. The ſame thing happened to an iron wire, which had been ſuſpended in the air for a great while.

I made another obſervation upon the courſe of the magnetic matter which paſſes through a great loadſtone, which is marked diſtinctly by the threads of the filings that are ſprinkled upon a paper placed upon the loadſtone, where we ſee that towards the poles of the ſtone, a great deal of filings are amaſſed, which form the origin of a vortex of which the bellies are between the poles, and that the filings are alſo in a pretty great quantity toward the edges of the ſtone, the threads of which proceed almoſt perpendicular from theſe edges; but there are but very little filings in the middle, which has different directions according to the inequalities of the ſtone, from whence we may judge that the ſtone has but very little force in its middle. The ſame thing is alſo obſerved in an animated iron

ron rod. This is confirmed by the experiment I ormerly made upon a ſteel ring of three inches liameter, which I had animated by only approaching the pole of the loadſtone againſt a part of this ing, for I knew by ſteel filings ſprinkled upon paper which I had placed upon this ring, the wo poles, one where it had been touched by the tone, and the other at the extremity of the diameter of the ring, which paſſed through the place where it had been touched. We obſerved alſo he bellies between theſe poles, but there did not ppear any thing ſenſible in the middle of the ring, which ſhewed me that the virtue of the loadſtone ad paſſed from one pole to the other, and had communicated itſelf the length of the ring without the bellies being confounded.

In effect the experiments that I have made upon he courſe of the magnetic matter, as has been ſeen n the preceeding figure, ſhews us that when we ave joined the two bits of iron to the ſtone all its virtue paſſes from its pole S, the furtheſt from the rons, to the pole m of the iron the fartheſt from he ſtone, and that toward the middle where the other iron is placed, there is but very little force; for the ſtone and both the irons have no other effect together than as a ſingle loadſtone; and I ave found this very plainly upon an excellent tone that was armed, where the magnetic matter eing all introduced into the armours, which were applied againſt the poles, turned itſelf wholly towards the heads of the armours, to make them ſuſtain a conſiderable weight; for hen the ſides of the armours overagainſt the poles had not any more ſenſible force.

It is almoſt the ſame here, where all the virtue of the ſtone paſſing from its pole S to the moſt diſtant pole *m* of the iron, can but very weakly hold

hold the middle iron, remaining fixed to the moſt diſtant iron which has received all the virtue of the loadſtone in the figure, which being long, has a good deal of force to act upon the middle iron that is near it; and this is what is marked by the threads of the filings, which come out almoſt perpendicularly from this iron, which gives it more force to join itſelf to the iron in the middle, than remains in the loadſtone to retain it.

I have before related, that when I made this experiment with the loadſtone A, and the two irons B and F, I had not obſerved that the iron in the middle remained fixed to the ſtone A when the other iron was removed, but on the contrary it always remained fixed to the other iron, and followed it: nevertheleſs M. *Rohault* relates, that this ſometimes happens, and this made me ſuſpect that the loadſtone A, having a good deal of force, communicated alſo a good deal of it to the moſt diſtant iron, or to the loadſtone of which it holds the place. For this reaſon, inſtead of this loadſtone A, I took a pretty long and thick piece of iron wire, which having been animated, became a proper loadſtone for theſe experiments; and inſtead of the other two irons B and F, I took a bit of iron wire about half a line thick, and three inches long, which I cut in two parts, one of an inch, and the other of two inches; and having animated theſe three pieces of iron, I placed them according to their poles upon a glaſs ſo that at firſt the ſhorteſt was in the middle, and they touched one another at their extremities. Theſe three irons then compoſed but one ſingle loadſtone, for they held faſtened together when moved, and it happened alſo, that when I drew away from the thickeſt iron, the longeſt of the other two, it drew along with it the ſhorteſt which

was

was in the middle, for the thickeſt was held faſt, which is conformable to the firſt experiments, for the thickeſt of the three communicated enough virtue to the moſt diſtant to make it retain that in the middle, which was the ſmalleſt; but when I would place the leaſt at the extremity, and the other, which was equal in thickneſs but longer, in the middle, I obſerved that ſometimes the ſmalleſt did not draw the other, away which ſeparated from it, and remained joined to the thickeſt of the three, which was immoveable, and this did not agree with the firſt experiments; but I can alſo ſay that the leaſt of the two thinneſt, which was the moſt diſtant, had not force enough to draw away the other which reſiſted it by its weight.

I obſerved alſo, by conſidering attentively the manner in which theſe wires apply themſelves againſt one another, to act upon it, that they do not join their circular extremities exactly one at the end of another; but turn themſelves a little that they may meet at the edges of their circumference, for they had been well prepared at the ends, which without doubt proceeded from the magnetic matters going out in greater abundance in this place, than towards the middle, as it happens to all bodies that are any thing large, when being animated we would make them ſuſtain a bit of iron, as we obſerve in a knife that is round at the point.

We muſt alſo obſerve that in the experiments there are ſome irons which cannot be animated, that is, which having been touched with a good ſtone, and being of a long figure, cannot ſuſtain a very light iron, at leaſt if it be not in preſence of a loadſtone, not that they are of a nature improper for that, but only becauſe their pores

cannot retain the virtue which had been impreſſed on them by the loadſtone.

But to conclude, we muſt conſider that the firſt irons applied to a loadſtone, make it a kind of armour which has a good deal of force towards its extremity, by gathering the virtue which is ſpread about the ſtone, which makes it act more powerfully than the ſtone itſelf; this is diſcovered by the armour, and it is this which makes it pull away from the ſtone the other irons that are near it, for this ſort of armour joins very ſtrongly to the iron which touches it, and which conſequently muſt carry it away with it when we would ſeparate them; this alſo may be ſeen by applying againſt either of the poles of the ſtone, an iron which ſerves it for an armour, without being fixed to the ſtone, for the extremity of this armour will join very eaſily to the iron that ſhall be preſented to it, ſo that they will ſeparate together from the ſtone, and that until the iron which touched the loadſtone is too far from it to receive force enough to retain the other, and this ſeems to me to be the true reaſon of the effect which we have undertaken to explain in this memoir.

VII. *An extract of the obſervations of the eclipſe of the moon*, September 20, 1717, *at* Nuremberg; *by M.* Wurtzelbaur.

The weather was favourable at *Nuremberg* for the obſervation of the eclipſe of the moon, *September* 20, 1717, which muſt appear above the horizon longer than at *Paris*, becauſe this city is ſituated with regard to us toward the eaſt.

At 6^h $5'$ $30''$ the upper limb of the moon appeared elevated a degree above the horizon, 3 minutes and $\frac{1}{2}$ before ſun-ſet, which is at 6 $9'$.

At

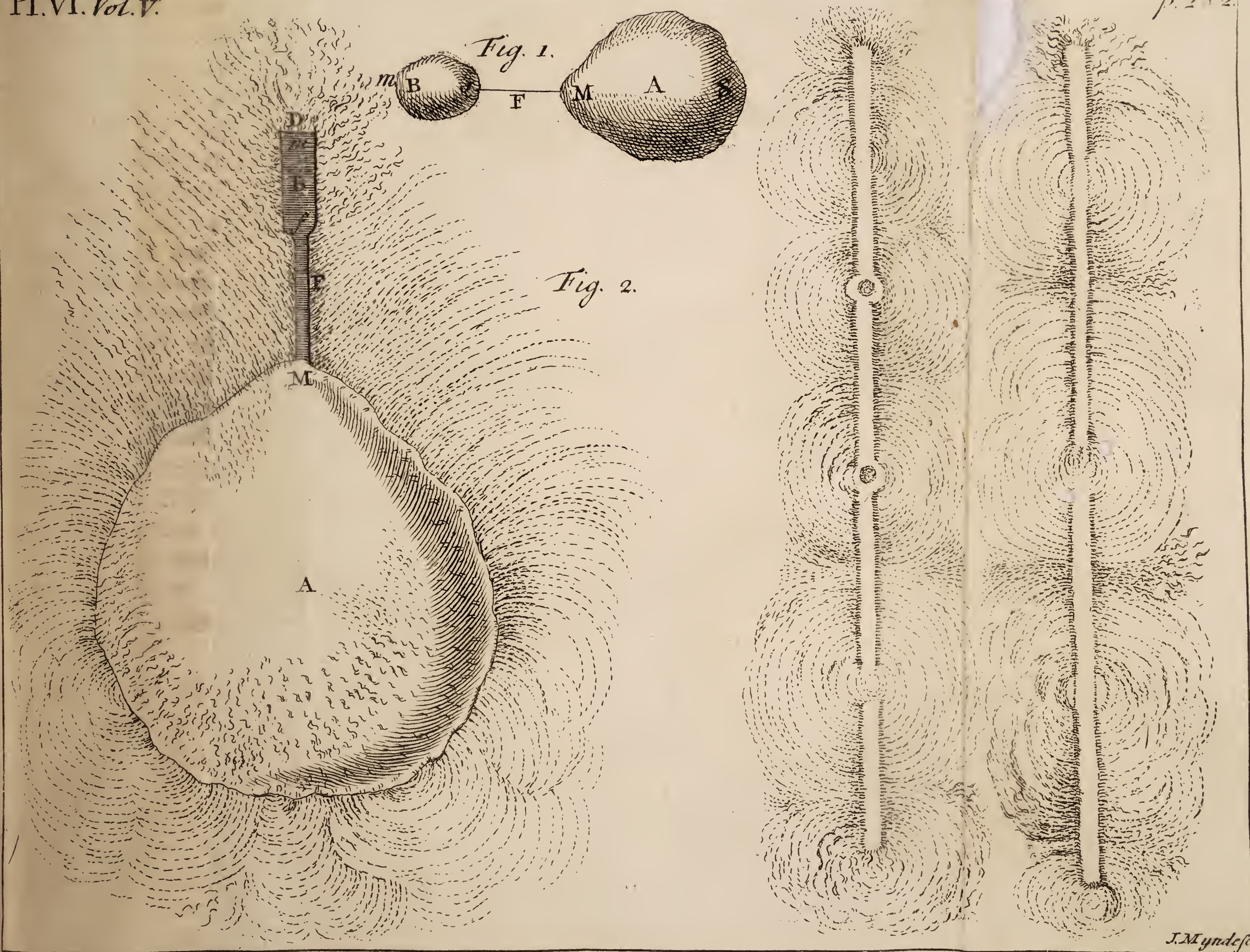


Fig. 1.
m B F M A S
Fig. 2.
D m B f F M
A
J. Mynde sc.

At $6^h\ 17'\ 30''$, the moon being almoſt entirely emerged out of the vapours, the magnitude of the eclipſe was obſerved to be almoſt 8 digits.

	h	′	″	
At	6	54	0	the eclipſe was ſeven digits.
	7	29	30	five digits.
	7	41	45	three digits $\frac{3}{4}$, *Tycho* is $\frac{1}{2}$ emerged.
	7	52	10	two digits.
	8	1	35	$\frac{3}{4}$ of a digit.
	8	10	45	end of the eclipſe at *Nuremberg*.

	h	′	″
The end of the eclipſe having been obſerved at *Paris* at	7	34	50
we ſhall have the difference of the meridians between *Paris* and *Nuremberg*,		35	55
within about 10″ of that which reſults from the obſervations of the ſpot of *Tycho*, which gives this difference		36	5

A TABLE OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXVIII.

In the HISTORY.

In the MEMOIRS.

IV

AN

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1718.

I. *On a question which relates to the theory of gravity.*

M. *Saurin* being engaged in the ſtudy of the theory of gravity, found it neceſſary to reſolve this queſtion: If two fluids, each of them homogeneous, having each of them all their parts equal, and of the ſame figure, for inſtance, ſpherical, and of the ſame denſity; that is, containing under the ſame bulk a like quantity of proper matter, both moved with the ſame velocity, and differing only in one of the parts being more groſs, and the other more ſubtile, have a different force againſt a plane that they meet?

One would at firſt make no difficulty in deciding for the affirmative, and that the ſubtile fluid muſt have leſs force. And yet M. *Saurin* found, contrary to his expectation and intereſt at that time, that the two fluids have an equal force.

Without repeating his demonſtration, which is ſo much the more exact and rigorous, as he was deſirous to be able to conteſt the conſequence it ſeems that the ſame thing may be proved by a pretty eaſy method. The two fluids being moved with the ſame velocity, have the ſame quantity

tity of motion, and conſequently the ſame *impetus*, if they have the ſame quantity of matter; and we are only to ſhew that they have.

I ſuppoſe a plane of a foot ſquare, and I put a ſphere of a foot in diameter, which touches the plane at its middle point, or at the interſection of the two diagonals. Here is the ſpace of a cubical foot as much filled as it can be by a ſphere. If I would fill this very ſpace with ſmaller ſpheres, and take them at firſt of half a foot in diameter, it is evident that it will require 8, for the ſquare plane being conceived to be divided into 4 equal and ſquare planes, each of half a foot on a ſide, each of them will bear its ſphere of half a foot in diameter, all theſe 4 ſpheres will fill but half the cubical ſpace of a foot, which filled the firſt ſphere alone; and to fill it as much as it can be, we muſt put upon theſe four ſpheres four other equal ſpheres. Now the ſolidities of the ſpheres being as the cubes of their diameters, each ſphere of half a foot will be $\frac{1}{8}$ of the firſt ſphere; and the 8 together will be equal to it: whence it follows, that there is always the ſame quantity of matter in the ſuppoſed cubical ſpace, whether it be filled by the ſingle ſphere or by the 8. In like manner, if we would fill it with ſpheres of $\frac{1}{3}$ of a foot in diameter, we ſhall find that 27 are required; each of which being 1 of the ſingle ſphere, the 27 will be equal to it, and always ſo.

It is viſible that theſe ſpheres, which always fill the ſame ſpace, which are always of the ſame matter, or equally denſe, and which only continually leſſen in diameter, repreſent a fluid, which we conceive always more ſubtile in the conditions mentioned. Thus the ſubtilty of a fluid takes away nothing from the force of its *impetus*; pre-

ſumptions,

ſumptions, even philoſophical ones, are often deceitful.

II. *On animals ſeen by the microſcope.*

There was reaſon to be ſurprized the firſt time we ſaw animals as ſmall as a wheale-worm or cheeſe-mite; and above all, if we conſidered what their organiſation muſt be, and of what a prodigious number of machines, leſs and leſs, to infinity, ſuch a ſmall machine is compoſed. It is not very probable, that the moſt philoſophical imagination would have gone ſo far as to ſuſpect ſuch animals in nature: but obſervations have made us ſo familiar with theſe ſorts of wonders, that animals 27 millions of times leſs than a mite no longer ſurprize us. Such is the enormous ſmallneſs of thoſe which M. *Malezieu* has obſerved with his microſcope; he has proved it by the geometrical calculation of the augmentation which this inſtrument cauſes in objects. What therefore are the bounds of the minuteneſs of animals? Our eyes reach from the elephant to the mite, and there a new order begins, reſerved for the microſcope, reaching from the mite to animals 27 millions of times leſs: this order is not exhauſted, if the microſcope is not yet arrived at its greateſt perfection; and when it ſhall be ſo, will the animals be exhauſted? There is on the contrary an extreme probability that they will not; the bounds of nature will not fit ſo juſtly with thoſe of our eyes aſſiſted by the microſcope. Who could even affirm that there are any bounds? All that can make us believe that animals have any in ſmallneſs, is that that they have them in greatneſs. On this ſide they are bounded by the elephant, and

and from thence to the leaſt animal exiſting, there is a progreſſion terribly decreaſing.

M. *de Malezieu* has made a ſingular obſervation upon the almoſt infinitely ſmall animals, which the microſcope diſcovers in drops of liquor; he has plainly perceived ſome of them to be oviparous, and others viviparous. Several of them are ſo tranſparent, that one may evidently diſtinguiſh through their outer ſkin their different *viſcera*, the motions of thoſe parts, and even the contrary motions of the blood, or liquor inſtead thereof, ſo that the circulation is ſeen at once through the whole animal. Of theſe tranſparent animals, M. *de Malezieu* has ſeen ſome lay eggs, which before were little grains, that might be counted in their inteſtines, and as ſoon as they were emitted became animals, which grew more like their mother every inſtant, as they unfolded and grew. The others produced ſmall living animals, which were ſo whilſt in the belly of their mother, and had diſtinguiſhable figures and ſenſible motions. We ſee by this, that the law of the generation of animals is very conſtant, and that nature is always the ſame both in great and little.

III. *On the mewing of cray-fiſhes.*

The marvels of cray-fiſhes have not been ſufficiently inquired into or exhauſted in 1709 and 1712 *; and indeed what part of nature can be? M. *de Reaumur* has examined anew the mewing of cray-fiſhes, that is, the change of the ſhell or boney covering, which happens to them every year, and he has found ſtill more novelties. He has ſhewed the academy ſome deciſive experi-

* Vol. III. p. 244, of this abridgment.

ments, which no prepoſſeſſion to the contrary can ward off.

It is in *June*, *July*, and *Auguſt*, that the cray-fiſhes quit their old ſhell, which remains empty, but all in a piece, that is, the different pieces which compoſe it are ſtill united as they were before; ſo that the ſpoil of the cray-fiſh looks as if it was an entire animal.

This ſpoil does not get looſe of itſelf, and without effort, but the animal aſſiſts it by different motions, which ſeem very painful. Sometimes it dies in the operation. M. *de Reaumur* gives the exact hiſtory of the ſucceſſive mewing; for we may well judge, that the animal has more eaſe to diſengage itſelf at firſt from certain pieces of its ſhell, and that it begins with thoſe, which afterwards draw others after them, or render them more eaſy to get looſe. In ſhort, it wiſely obſerves the order of its greateſt convenience.

There is one circumſtance in the mewing, which might ſeem inconceiveable. A leg gets from within its boney ſheath, and the only aperture in this ſheath, through which the leg could paſs, is ſo ſmall, that it is impoſſible it ſhould paſs through: nor indeed did it, but the ſheath has an inviſible ſlit lengthwiſe, and is compoſed of 2 pieces ſo exactly joined, that they ſeem abſolutely to be but one. It opens to let the leg get out at the ſlit, and at that moment ſhuts itſelf ſo cloſe, that the ſlit entirely diſappears, as if nature had ſtudied to hide the induſtry which it uſes to make this leg get out.

The cray-fiſh therefore remains naked, and covered only with a ſoft membrane; but in two or three days at moſt, this membrane becomes a

boney

boney covering, entirely like that which fell off, but larger.

It is larger, becauſe in the ſpace of a year the cray-fiſh has grown, and requires a larger veſture. According to M. *de Reaumur*'s opinion, this animal mews every year, becauſe it grows itſelf, and its coat does not. In ſhort, if we compare the animal at the time of its being naked, with the ſpoil it has juſt quitted, we always ſee that it could hold it no longer, and that a new coat was neceſſary. The cray-fiſh grow ſlowly, perhaps becauſe of the conſtraint of the bony covering; and the fiſhermen ſay that at 6 or 7 years of age it is ſtill but of a moderate bigneſs.

M. *de Reaumur* ventures to gueſs at the manner in which the ſoft membrane, which cloaths the naked cray-fiſh, changes to a ſhell. What are called crab's-eyes, are two little ſtones ſeated in the ſtomach of the animal, which are not found there at all times of the year, and are found in different ſtates of the growth. M. *de Reaumur* has obſerved, that theſe ſtones are biggeſt exactly when the cray-fiſhes are ready to mew; and that when the new ſhell has acquired all its hardneſs, there are no more ſtones. In the intermediate ſpace, which is very ſhort, they only decreaſe. Hence he ſuppoſes they are a ſort of quarries, whence the ſtoney matter proceeds, which is to harden and petrify the membrane with which the cray-fiſh is covered. When this is done, the quarries are exhauſted, and they begin to fill again during the following year. There is ſomething ſo juſt in this, that it is hard to think it only a ſport of imagination.

An explanation of the figures in Plate VII.

Fig. 1. is a cray-fiſh, which has already looſened the firſt of the tables from that part of the ſhell, which M. *Reaumur* calls the *caſque*. Aaa the *caſque*, bb the firſt of the tables.

Fig. 2. is a cray-fiſh juſt ready to mew; ccc the edge of the *caſque*, which is very much raiſed, and is at preſent found at a great diſtance from the firſt table dd. The part ee, and all that is between cc, dd, is at preſent open.

Fig. 3. The ſpoil of a cray-fiſh which has mewed; h the *caſque*, i the firſt table, k the place where the cray-fiſh drew out its head. The edge h of the *caſque* would have been applied againſt the edge i of the firſt table, if we had repreſented theſe ſhells in the ſituation that they take when the cray-fiſh has juſt quitted them, but they are repreſented juſt as they are placed at the inſtant when the cray-fiſh is juſt ready to get out.

Fig. 4. is the ſame with *Fig.* 3. only the *caſque* is removed, to ſhew how many different parts remain faſtened to the ſpoil.

Fig. 5. is part of a leg of a cray-fiſh in the ordinary ſtate; l l marks the ſtraiteſt place of the leg contained between the ſecond and third articulation.

Fig. 6. is the ſame leg, when the fleſh which occupies the large end of the ſhell is arrived at the ſtrait part marked l l in the preceding figure. We here ſee the ſhell open, and the fleſh nn breaking out, after having forced the ſuture m m of the ſhell.

Fig. 7. is the ſame leg in another poſition. We ſee here beſides, that the ſhell is opened at

at P. OO here repreſents the ſame fleſh that is marked mm in *Fig.* 6.

IV. *Of a lizard with two tails.*

M. *Marchant* having perceived in his garden a grey lizard with two tails, killed it that he might have the diſpoſal of it, and might examine it at leiſure. There was nothing ſingular in it but the two tails. One, which, as it ſeemed by its direction, ought to have been the only one, was ſomething thicker than the other, but ſhorter. It alſo appeared to have been cut toward the extremity, for it did not end in a ſlender point as it ought to have done, but in a pretty thick and obtuſe one. It was but 16 lines long, and the tails of theſe animals are uſually three inches and more. It was a little flatted at top, and almoſt quite ſtrait. The ſecond, which was ſituated to the right of the firſt, threw itſelf to the right, and bended outward. It was 22 lines long, and 2 in diameter at its origin, equally round on all ſides, and terminated in a ſharp point.

A lizard is covered with bands or cinctures, from the hinder feet to the end of the tail; and M. *Marchant* has obſerved, that theſe bands, which ſeem to be ſeparated, and compoſed of ſcales, are however only a continued ſkin, but folded in ſuch a manner, that the different plaits or folds cover one another, and it is this which makes the cinctures. It was from the edge of the laſt cincture upon the body of the lizard obſerved by M. *Marchant*, that the two tails grew.

One might even ſuſpect the riſing of a third. It was a little appendage 2 lines long, and ½ a line in diameter, ſituated 2 lines above the bifurcation of

of the two tails, and it ſeemed by its ſtructure as if it would have grown to one.

On diſſecting the animal it was found, that whereas in the common lizards the tail is formed by little boney *vertebræ*, which make it very brittle, in this the two tails; as alſo the origin of the third, if it was really one, were formed only of cartilages, which rendered them leſs brittle and more flexible.

Ariſtotle relates, that if the tail of a lizard or ſerpent is cut off, it returns again, and after the manifeſt reproduction of the legs of cray-fiſhes, already ſpoken of, this fact may be eaſily admitted. M. *Perrault*, in his *phyſical eſſays*, ſays, that the tail of a green lizard being cut off came again, and that inſtead of the *vertebræ* there came a cartilage of the ſize of a large pin.

Though the lizards of *Ariſtotle* and M. *Perrault* were not quite in the ſame caſe with that of M. *Marchant*, he had a mind to try the reproduction of the tail in grey lizards, ſuch as his. But the experiment did not ſucceed with him, and he has not been able to diſcover what could hinder it.

What comes up exactly to his obſervation, is what *Pliny* ſays, that there are lizards found with double tails. *Johnſton* and ſeveral others have alſo advanced it, but they omit ſeveral neceſſary circumſtances. Thus we muſt keep to a fact well atteſted, and time will inform us of more.

V. *Of a monſtrous large egg found in the cavity of a hen's belly.*

A young hen, which had been accuſtomed to lay pretty large eggs, growing more and more languid every day, and alſo barren, M. *Morand* the

the ſon, ſurgeon to the hoſpital of invalids, who ſaw it reduced to extremity, ſhortened its life a few hours to examine the cauſe of its diſorder. He found a great tumour faſtened to the meſentery by a pedicle, and in this tumour a monſtrous egg, which weighed 14 ounces wanting 2 drams, whereas the common weight of the largeſt eggs is but 2 ounces; that is, it weighed near ſeven times as much. The pouch, in which it was incloſed, being very big and heavy, and incloſed in the cavity of the belly, had extremely incommoded the inteſtines, and cauſed a diſorder in the animal, which increaſed every day.

The egg had its white and its yolk: the white was very much hardened, and one might count 36 diſtinct *ſtrata* in it, the yolk on the contrary was more fluid and pale than uſual.

According to M. *Morand*'s conjecture, who brought this obſervation to the academy, the egg had been fertilized, and falling from the ovary into the oviduct, it grew extraordinary thick in this duct by ſome particular cauſe, burſt it, and fell into the cavity of the belly, where it was faſtened to the meſentery by the pedicle, by which it had been faſtened to the ovary, and which it carried along with it. It had found a ſuitable nouriſhment in the belly, and had grown very big there. The heat of this part, where it had tarried a long time, had hardened the white to a degree which no artificial dreſſing ſeems to have been able to imitate; and as it is the white that nouriſhes the yolk, this white being too much hardened, could no longer furniſh the neceſſary juices to the yolk, which thereby had loſt its natural conſiſtence, and was diſſolved.

This fact is remarkable in its analogy to the human *fœtus's*, which are nouriſhed in the tubes, or

or in the cavity of the *abdomen*; the ſame accidents, and the ſame errors of nature, may happen both to the oviparous and viviparous animals.

VI. *Of the* ſperma ceti.

M. *Lemery*, in his treatiſe of ſimple drugs, has ſet all the naturaliſts right with regard to their miſtakes about the *ſperma ceti*, by informing them that it is the brain of the whale prepared after a certain manner. To this information, which we have had but 30 or 40 years, M. *de Juſſieu* adds another, which he had from M. *Weils*. The *ſperma ceti* is only the brain of thoſe whales which have teeth, and they are but rarely found. They have 32 of them, and theſe teeth weigh 1 or 2 *lb.* apiece.

When the *ſperma ceti* is found floating upon the ſea, it is therefore the brain of theſe animals dead and putrified, which has received from the ſun and waters a preparation equivalent to the artificial.

VII. *On the ancient and modern* China.

The duke of *Eſcalona*, foreign aſſociate of the academy, has ſent them a map of *China*, drawn by a *Chineſe* hand, and in *Chineſe* characters, and very different from ours. There are no degrees of longitude or latitude; the conſiderable towns are repreſented by ſquares, perhaps becauſe the *Chineſe* have affected this figure in building them; it is certain, from all accounts, that *Pekin* is ſquare, and that they believe the earth to be ſquare. They may have formed this notion from *China* being nearly ſo; for, according to them,

China

China is every thing of importance upon the earth.

M. *Deliſle*, to whom this map was referred, having given it to M. *de Fourmont* to be ſtudied, who carries the knowledge of languages even to the *Chineſe*; he knew from him that it contained the ancient and modern names of the principal towns, and that at the bottom there was an enumeration of the tributes of each province, whether in ſilver or in merchandiſes. The neighbouring people to *China* are thrown as it were at random at the edges of the map, and are expreſſed by the names of *monſtrous men*, giants, dwarfs, *&c.* as if they were not worthy to be called by their real names.

As this *Chineſe* map may be of great ſervice in comparing the ancient *China* with the modern, M. *Deliſle* has endeavoured to draw ſome information from it. For example, it is marked therein, that the province of *Chanton*, which is in the northern part of the empire, furniſhes ſilk, and thence he conjectures that it is the *Serica* ſo famous among the ancients.

It is true that *Ptolomy* diſtinguiſhes *Serica* from the country of the *Sinæ*, which muſt be the modern *China*; but it is very poſſible, that in the time of *Ptolomy*, they might give the name of the country of the *Sinæ* only to the ſouthern part of *China*; and in effect he places at the 35th degree of latitude the limits of his *Serica*, and of his country of the *Sinæ*, which is more to the ſouth; and it is at this very degree, within about 15′, that our modern obſervations place the limits of the province of *Chanton*, and of that of *Nankin*, which without doubt was contained in the province of the *Sinæ*.

It may not be amiſs to obſerve, that *Ptolomy* informs us himſelf, that it is towards the 36th degree of latitude, or towards the parallel of *Rhodes*, that they had the moſt obſervations in his time. It is eaſy to ſee the reaſon of it by the navigations that were then made; and it held alſo for the navigations that were undertaken in the eaſtern ſeas, more frequented towards the ſame parallel, becauſe of the merchandiſes that were there ſought. We muſt therefore rely upon *Ptolomy* for the poſition of the confines of *Serica*, and of the country of the *Sinæ*, and conſequently place *Serica* in the northern *China*.

However all the maps place *Serica* in *Scythia*; but very probably it is a miſtake, for *Ptolemy* does not place it there; and beſides *Serica* muſt produce ſilk, and there is none produced now in the *Scythia* of the ancients, which is our *Tartary*.

When *Ptolomy* is got out of the 35th or 36th degree, and in the country of the *Sinæ*, M. *Deliſle* finds him far from having the ſame exactneſs, probably becauſe the navigators and merchants did not well know the places where ſilk was ſold. He places the capital of the *Sinæ* in the 3d degree of ſouthern latitude, but by our modern obſervations there is no part of *China* nearer the equator than 18°; and by the annals of this empire, which run very high, we know all the cities that have been capitals by the abode of the emperors, and they are all found to be in the modern *China*.

And though according to the opinion of the late M. *Caſſini*, ſome countries as far as the equator, and beyond it, as *Siam*, *Cambodia*, the iſlands of *Borneo*, *Java*, &c. might anciently have depended on *China*, yet could we aſſign to this empire

pire a capital three degrees beyond the equator, and ſo far from the centre of the ſtate.

Laſtly, of theſe iſlands of the *eaſtern archipelago*, which muſt have depended upon *China*, M. *Deliſle* thinks he has found ſome of the principal in *Ptolomy*, which he does not aſcribe to the *Sinæ*. For example, the iſland of *Jabadu* is pretty plainly the iſland of *Java*; we know that in the *Malayan* tongue *dive* means iſland; and beſides *Ptolomy*'s iſland extended from eaſt to weſt like *Java*. The ten *Maniolæ* of *Ptolomy* are the ten *Philippines*, which are alſo called the *Manilles*, a name not very different from the antient one.

From all this it reſults, that *Ptolomy* knew the *northern China* much better than the ſouthern, which he has extended exceſſively beyond its bounds. He has alſo erroneouſly placed on the other ſide of the equator the three iſlands of the *Satyrs*; if according to M. *Deliſle*'s conjecture, they are the three iſlands of *Japan*. It is not very ſurpriſing that the ancients ſhould be miſtaken in theſe points, but rather that they ſhould have a great number of pretty juſt and exact informations.

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1718.

I. *Meteorological observations made at the royal observatory, during the course of the year* 1717; *by M.* de la Hire*.

THE quantity of water which fell in rain or melted ſnow, was in

	Lin.		Lin.
Jan.	7 ½ ⅛	July	25 ⅛
Feb.	9 ¾ ⅛	Aug.	14 ¼ ⅛
March	7	Sept.	26 ½ ⅛
April	17 ½	Oct.	10
May	20 ¾ ⅛	Novem.	15 ½ ⅛
June	18 ¼	Dec.	39 ½ ⅛

Which gives for the whole year 212 lines ½, or 17 inches 8 lines ½, which is a little leſs than the common years, which we have ſettled at 19 inches. The ſummer rains alſo were but moderate, contrary to cuſtom, for the three months of that ſeaſon furniſh almoſt as much as all the reſt of the year. However the month of *December* has given almoſt 40 lines. There fell but little ſnow on the night of *January* 1. and though the fertility of the earth is aſcribed to the ſnow which fattens the earth, as they ſay, yet the harveſt was very plentiful.

* Jan. 8. 1718.

There

There were a great many fogs during the whole year, which have made up for the rains and alſo for the ſnows.

July 4, there was a ſtorm with much hail of a very large ſize, for ſome of it was 7 lines in diameter, which did a great deal of injury to the trees and fruits in the places where the clouds paſſed.

My thermometer was at the loweſt this year, at 24 parts *February* 13, which does not indicate a great cold, for it often falls to 15 parts, and it begins to freeze in the country when it is at 32 parts, towards the end of this year it fell no lower than 31 parts *November* 27.

It roſe at the higheſt to 65 parts *Auguſt* 1, but it muſt here be obſerved in general, that about ½ an hour paſt two in the afternoon, it riſes 12 or 13 parts higher than in the morning at ſun-riſing. We may therefore take for the greateſt heat of this year, that marked by my thermomer at 78 parts; whence we may conclude, that the heat this year was greater than the cold; for to make the cold equal to the heat with regard to the mean ſtate, the thermometer ſhould have fallen to 18 parts, inſtead of 24.

We now proceed to the barometer, which is an inſtrument that ſerves us to meaſure the weight of the air. That which I always uſe in my obſervations is the ſimple one. It is commonly believed that this inſtrument can point out the alterations of the weather, ſome hours after the quickſilver has changed its height in the tube wherein it is ſuſpended, and it is pretty often found, that when the quickſilver falls, it is a ſign there will be rain, and on the contrary, when it riſes, that the air will become more clear. However, when it falls, it is a certain ſign that the air becomes lighter, and on the contrary that when it riſes the air becomes heavier.

heavier. But it very often happens, that we are deceived in these predictions of rain and fair weather. I have explained in some preceeding memoirs, whence this may come, and why we must not depend too much upon the signs of the barometer. Mine is always situated in the same place, at the top of the great hall of the observatory. I have another, in which the quicksilver rises 3 lines higher than in the common one.

I found my common barometer at the highest at 28 inches 2 lines $\frac{1}{6}$ *February* 10, and *December* 29, in weather that was not very fair; and it was at the lowest at 26 inches 10 lines $\frac{1}{3}$, *February* 22, and *March* 12. It rained moderately in *February*, and a little in *March*. The difference between the greatest and least height of the berometer was therefore 1 inch 5 lines $\frac{1}{6}$, which is very nearly like the other years.

The most common and violent winds of these countries commonly come from the S. W. and they very often bring rain, because there arise more vapours from the sea, whence they are brought, than when from the E. and we always observe that the clouds considerably increase the force of the wind, which might be caused by the wind, which being of itself but moderate, and being compressed between the clouds and the earth, considerably augments its velocity, and in foggy weather there is hardly any wind. We often observe, that the upper winds, which we know by the motion of the clouds, are very different from those which prevail upon the earth, and I once saw upon the earth, two winds opposite to one another, at the distance of about two leagues, which may be ascribed to some mountains that turned the same wind different ways.

The

The ſtrongeſt winds of this year were in *December*, when there was a great deal of rain, though the barometer was only about its mean ſtate.

I obſerved the declination of the needle *December* 29, with a needle of 13 inches ½ in length, in my ſtone box, which I deſcribed laſt year, and I found it 12° 40′ from the north toward the weſt, to which it tends more and more every year, but I have remarked that this obſervation was very eaſily made, and that it muſt be very exact, becauſe in ſhakeing the box conſiderably, after the needle was fixed to a point, it returned to the ſame point afterwards, having made ſeveral vibrations, which does not commonly happen, eſpecially with great needles. I have not found any better reaſon for this effect than the great calm that was then; which makes me ſay, that to obſerve this declination well, we muſt chooſe a calm time, for in other weather, tho' the box is very cloſe, I have not obſerved the ſame thing; and yet I cannot be perſuaded that the agitation of the air can cauſe any in the magnetical matter, which directs the needles of the compaſſes.

II. *An obſervation of a northern light; by M.* Maraldi*.

There appeared in *March*, this year 1718, a great northern light, like that which was obſerved in *Europe* in *March* and *April* 1716.

I began to perceive it the 4th of *March* at $7^h \frac{1}{4}$, after having obſerved the entrance of the 4th ſatellite into the ſhadow of *Jupiter*, and it continued to appear till $8^h \frac{1}{2}$, having laſted above an hour. That night the heaven was very clear, except on the N. ſide, where there was a ſort of thick fog,

* March 23, 1718.

mixed

mixed with ſome clouds, which came out of the horizon, and roſe to the height of about 3 or 4 degrees.

The light was diffuſed in the upper extremity of theſe fogs from N. E. to N. W. declining about 10 degrees more to the N. W. than to the other ſide, ſo that it extended through about 90 degrees of the horizon.

It was very bright like the dawn of the day, and was formed into an arch, the convexity of which was toward the zenith, its greateſt height being nearly at an equal diſtance from the two extremities, which terminated in the horizon, one toward the N. E. and the other toward the N. W.

Its concavity was terminated by the fogs which were near the horizon, and in its upper part it arrived at the beginning, almoſt to the bright ſtar, of the ſecond magnitude, in the tail of the *Swan*, which was elevated 8 or 9 degrees above the horizon; this was its leaſt height, for it increaſed conſiderably by intervals, riſing ſometimes to the head of the *Dragon*, which was 11 degrees high, and and afterwards falling 2 or 3 degrees lower, ſo that it had always a breadth of 5 or 6 degrees, and ſometimes it had one of 7 or 8.

We could eaſily ſee through this light not only the bright ſtar of the tail of the *ſwan* of the ſecond magnitude, but alſo thoſe of the third and fourth degree of magnitude, which are in its northern wing, and thoſe of the legs of *Hercules*.

From $7^h \frac{1}{4}$ to $7^h \frac{3}{4}$ there happened no other alteration in the light than that of dilating and contracting itſelf by intervals. But a little before 8 there was formed a luminous arch, about 2 degrees broad, ſeparated by a little ſpace, and more elevated by ſome degrees, than the upper extremity of

of the light, which always preſerved the ſame brightneſs.

Soon after this arch was formed, there appeared a ſecond above the firſt, which had almoſt the ſame breadth and brightneſs, except its being interrupted in ſome places. This laſt arch was about 45 degrees elevated above the horizon, and was ſeparated from the firſt by a great ſpace.

Theſe two arches laſted but a little time, and ſoon after 8, $\frac{1}{4}$, when they were diſſipated, we ſaw through the whole extent of the horizontal light, a great number of columns of light, which proceeded from the clouds, and croſſing the horizontal light, roſe perpendicularly to the height of about 25 degrees.

When theſe columns ceaſed to appear, the horizontal light loſt its brightneſs, and diminiſhed till $8^{h}\,\frac{1}{2}$, when it was no longer viſible.

III. *On the rivers and rivulets of* France, *which carry gold duſt; with obſervations on the figure and manner of collecting ſuch duſts, and on the ſand mixed with them; by M.* de Reaumur; *tranſlated by Mr.* Chambers*.

The new world has ſent the old one ſuch ſtore of gold and ſilver, eſpecially after the firſt diſcovery thereof, that we are uſed to conſider it as the native country of thoſe metals. Dazzled with the riches brought us from *Peru* and *Mexico*, we have almoſt forgot that the reſt of the world could formerly furniſh enough to ſupply, both the occaſions of commerce and luxury; yet the *European* mines, eſpecially thoſe of *Germany* and *France*,

* April 27, 1718.

were very rich, as had been abundantly proved by *Agricola* in his treatise *de veteribus & novis metallis.*

Notwithstanding our aversenefs to dig in the earth, we have still some silver mines in the kingdom not utterly abandoned; and as to *Germany* there are several not only of that metal, but even of gold, continue to be wrought with succefs. M. *Stahl*, a very able chymist, puts some of these countries on a level with the richest in either *India* in this kind of produce; and is downright angry with those who disbelieve it. He asserts that within these 400 years upwards of 40 thousand millions of silver, besides many tons of gold, have been procured out of the mines of *Germany* alone. It will be to little purpose to add that the poets have given the epithet *Auriferi* to several of our rivers, and even to some in this kingdom, and yet some of the rivers in *France* highly deserve this title, for instance the *Ariegé*, which derives its name from *Aurigera.*

It is true our riches in this kind are not very great, and the gold now gathered in our rivers is hardly enough to subsist the peasants who are employed therein, for a few months in the year; but they are at least sufficient to raise the curiosity of a naturalist, and perhaps engage him in a search which might be more advantageous.

1st. The *Rhine* holds one of the first ranks among the rivers which convey gold dust with their sand. Not that the dust is here more copious and larger than in some other rivers of *France*, but it has the advantage of running through an industrious country, where they make the best of their mineral productions, and where the *Metallurgia* is carried further than in any other part of *Europe.* Gold-dust is found among the sand of this river, from

from *Strasburgh* to *Philipsburgh*, it is ſcarcer between *Strasburgh* and *Briſac*, by reaſon the *Rhine* running more rapidly there, carries the greateſt part of its gold further. The place where it depoſits moſt is between *Fort Louis* and *Germeſhiem*, the right of gathering it belongs to the lord of the grounds through which it paſſes. The magiſtracy of *Strasburgh* have it for about two leagues of the river, and let it out on condition that they who gather the gold, ſhall ſell it them at 16 livres the ounce, which they afterwards ſell to the goldſmiths at a higher rate. The truth is, their view in letting it out, is rather to preſerve the right, than for any profit they make of it, there being hardly four or five ounces of gold brought them in a year. The biſhop of *Strasburgh*, the earl of *Hanau*, and ſeveral other lords likewiſe farm it out in their reſpective domains on other conditions. The people employed in ſeeking it commonly gather from 30 to 40 ſols per day, it may be ſaid that the workmen in the mines of *Peru*, are not paid near ſo well in proportion; the miſchief is that our labourers are but few in number, and only work a ſmall part of the year.

2d. The *Rhone* in its paſſage through the *Pays de Gex* affords gold enough among its ſand to employ a number of peaſants all winter, who earn from 12 to 20 ſols per day. The chief buſineſs is to raiſe huge ſtones, and take off the ſand ſticking about them, from which ſand they pick the duſt. It is not certain whether the *Rhone* affords this duſt from its own ſtock, or whether the river *Arve* do not import it with its waters into the ſame as we only find it from the mouth of that river, to the diſtance of 5 leagues below, at leaſt it is certain it does not bring the gold from its ſource, ſince in

that caſe it muſt lodge them in its paſſage of 22 leagues through the lake of *Geneva.*

3dly. The river called *le Doux* running through the *Franche Comté*, hardly deſerves to be paralleled with the rivers abovementioned; its ſand is intermixed with grains of gold, but thoſe in ſuch ſmall quantity, that they are never looked for unleſs out of curioſity.

4thly. But the *Ceze* though a ſmall river comes nothing behind either the *Rhine* or *Rhone* in the quantity of its gold-duſt. It riſes near *Villefort* in the *Cevennes*, and for ſeveral leagues of its courſe affords gold-duſt, in equal number and commonly much larger in bulk than either of thoſe rivers, and the time ſpent in ſeeking them is commonly much better paid; for on a lucky day they will earn upwards of a piſtole, though frequently they go without any grains at all.

5thly. The river *Gardon* which likewiſe ariſes from the mountains of the *Cevennes*, brings with it gold-duſt about the ſame rate with the former.

6thly. The *Ariege* is by no means to be omitted, its very name claiming a place for it in the preſent liſt. It affords gold in its whole paſſage through the *Pays de Foix*, but in greateſt plenty about *Pameers*; where it beſt quits the coſt of ſeeking. It alſo yields a little in the biſhoprick of *Mirepoix.*

7thly. A little crop of gold duſt is alſo gathered yearly in the *Garonne*, a few leagues from *Thou'ouſe*, but there is room to ſuppoſe that it owes them chiefly to the *Ariege*, it being below the confluence of this laſt river that they commonly ſeek it.

8thly, and 9thly. The *Ariege* itſelf may have perhaps received a great part of its gold from elſewhere; ſince ſeveral little rivulets which run into it

it are found pretty rich in this way, especially those of *Ferriet* and *Benagues*; both of which come from the heights on the left, as we descend from *Varilhere* to *Pamiers*.

10thly, The *Salàt*, a little river, whose spring, like the *Ariege*, is in the *Pyreneans*, and runs through the county of *Couserans*, and the generalty of *Pau*, rolls gold-dust enough with it to employ the peasants about *St. Girons* some part of the year in gathering it.

We may therefore reckon ten rivers or rivulets in the kingdom, which yield gold-dust enough to be worth the gathering. It is true the crops are but small, and it were better to exceed other countries in the quantity of the matter, than in the number of places where it is found. The advantage is visibly on our side in this last respect; there being few countries of the compass of *France*, where there are near so many gold-bearing rivers, an advantage which we have always had, and which was much better known heretofore than now. *Diodorus Siculus* mentions it as a privilege peculiar to *France* to afford gold, without the necessity of seeking or working it by art. He adds, that the antient *Gauls* had a method of procuring gold from their river sands by lotion, and that they melted it down and made rings, bracelets, girdles, *&c.* thereof. *Galliam omnem sine argento*, says he, *sed aurum ei a natura datum sine arte & sine labore, propter arenas mixtas auro, quas flumina extra ripas diffluentia montesque longo circuitu per montes ejiciunt in finitimos agros, quas sciunt lavare & fundere, unde homines & fœminæ solent sibi annulos, zonas, & armillas conficere.*

We shall not swell our list with the gold rivers mentioned by several authors; we omit the *Farne*, notwith-

notwithſtanding the authority of *Auſonius*; and the little river *De Giers* ariſing on *Mont Pila*, notwithſtanding the explicite account given us by *Du Choul* of the manner of gathering its gold. We alſo omit the river *De Chenevalet* running through *Foreſt*, notwithſtanding what *Papirius Maſſo* has related of it: nor ſhall we dignify the *Lot* and *Gaves* of the *Bearnois* with the title of *Auriferus*: how much ſoever has been ſaid in their behalf on account of theſe, may hereafter help to make a hiſtory compleat, whereof we now only give an eſſay.—Our deſign was to admit none into the rank of golden rivers, but thoſe which are really ſo, and which we have either found ſuch by our own experience, or by the moſt undeniable teſtimonies, the duke of *Orleans* having ſent ſeveral orders to Meſſ. *De Baville*, *d'Angervilliers*, *De la Briſſe*, and *Dandrezel*, to gather, with all poſſible care, the ſeveral ſands and duſt of the *Rhine*, *Rhone*, *Ceze*, *Gardon*, and *Ariege*, which run through the reſpective generalties, and tranſmit them hither; upon which we have made obſervations, which ſhall be delivered in the ſequel of this memoir.

To give room however for further diſcoveries on this head, we ſhall add a word about the places and ſeaſons moſt ſuitable for ſuch enquiries.—The rivers which yield gold-duſt, do not produce it in the places where we now find it, but have brought it hither from elſewhere: if this need any proof, it may have it when we come to examine their figure. It is either the torrents or brooks therefore which empty themſelves into the river, that inrich them with the gold they have gathered; or the rivers themſelves muſt have ſwept it up in their courſe; the more rapid the courſe of the river is, the leſs eaſy will it be for the

the duſt to precipitate, and the water will drive them along till they be deeply enough engaged in the ſand to reſiſt its effort; and hence in ſome meaſure it is, that all parts in the courſe of a river have not equal ſhares thereof, there being moſt found in thoſe places where the ſtream runs ſloweſt, and the bed is wideſt, more eſpecially in thoſe lets where the water begins to loſe its velocity, and near the elbows or angles where the direction of the ſtream is altered; it may be added, that the ſtones at the bottom of rivers have ſomewhat the effect of dikes, and ſerve to ſtop the duſt. Accordingly it has been already noted, that the peaſants who ſeek duſt in the *Rhone*, gather the ſand which fixes round the ſtones with great care; for the reſt there is no occaſion to dig deep; the duſt-ſeekers in the *Ariege* never go above two foot, but uſually content themſelves with taking four inches from the upper ſurface of the ſand.

The proper time for this ſearch, is when the waters are low, which gives opportunity for taking ſand further off the edges. For this reaſon it is, that the peaſants about the *Rhone* ſeek chiefly for duſt in the winter time, when the river is commonly very low; but of all times the moſt favourable is that when the water comes to be low preſently after an inundation. Rivers and torrents act more forcibly upon the ores they paſs over while their waters are ſwelled, and thus ſeparate more particles therefrom, which withal are not ſunk ſo deep in the ſand, if care be taken to gather them ſoon after the water is retired. The peaſants about the rivers *Ceze* and *Gardon* never baulk theſe promiſing occaſions. The produce of the *Ariege* laſt year was leſs than uſual, by reaſon its water was always low. The grains

of

of duſt are frequently ſo ſmall, and ſo thin ſpread withal, that they eſcape the ſharpeſt and moſt attentive eye. The method therefore is to obſerve where the ſand is of a blackiſh or reddiſh colour, or of any colour in general, ſomewhat different from what it is elſewhere. This ſand they faſten on, and finding it to contain gold, or more plenty of gold than any of the reſt.

We come now to the manner of ſeparating the duſt from the ſand, which ſeems a work fit only for thoſe fabulous ants in the *Indies*, and could hardly be expected from the induſtry and addreſs of mankind, did we not actually find it effected thereby. A whole buſhel of ſand ſhall ſometimes only contain two or three grains of gold about the ſize of a pin's point; and yet theſe two or three grains are ſought out and ſeparated from the reſt of the heap by a very ſimple operation. The idea we commonly have of the riches of *Peru*, will perhaps ſet our duſt gatherers in a pitiful light; but it ſhould be conſidered how parſimonious nature has been of this metal in every other country. M. *Frezier*'s *voyage to the* South-Sea will effectually cure us of this prevention. He tells us, that at *Copiago* in *Chily*, the *caxon* that is 5000 *lb.* weight of the richeſt ore, only yields about 12 ounces of gold, and that there are ſeveral which ſcarce yield 2 ounces the *caxon*, and conſequently hardly defray the expences of working. Now to ſeek for 2 ounces of gold in 5000 *lb.* weight of matter, is not much better than what our duſt gatherers have to do.

The chief part of their work conſiſts in repeated lotions; it is by waſhing the ſand that they diſengage the grains of duſt therefrom. This manipulation makes the baſe of the preparation of moſt ores, and has accordingly been deſcribed at large

large by *Agricola*, *Ercker*, and the other metalurgiſts. It may not however be amiſs to relate the methods of waſhing the ſand practiſed in *France*, which comes naturally enough within the ſcheme of the hiſtory of our gold-bearing rivers; and they who are not much converſant among the antients, will here find the chief circumſtances of that work.

Having placed the *Rhine* at the head of our liſt, we ſhall alſo begin with the method of waſhing its ſand.—After the waſher, which is now the name of our duſt gatherer, has pitched on a promiſing place at the edge of the river, he fits his little implements, which is done without much ado. The chief member is a board about 5 foot long, 1 and ½ broad, and 2 inches thick on each ſide; and at one end whereof is a little ledge about an inch and ½ high; then reſting the ledged end on the ground, he places the other on a treſsle about a foot and ½ high; and on this inclined board ſlightly nails three pieces of thick cloth, each about the breadth of the board, and a foot long. The firſt is faſtened at the upper end of the board, the ſecond within a foot of the firſt, and the third at a like diſtance from the ſecond. On the upper end of the board he alſo faſtens a kind of wicker baſket, whoſe bottom is oval, and its convexity turned towards the lower end of the board: this baſket is the firſt ſieve through which the ſand is to be paſſed, in order to ſeparate the ſtones and gravel therefrom.

Near this little machine he gathers a heap of the river ſand, and with a ſhovel fills the baſket therewith; after which with another ſhovel he takes up water, of which there is no want, and throws it into the baſket; the water diluting the ſand, carries it with it through the ſieve, into which

which the waſher continues ſtrowing new water till there be none left, but what is too groſs to paſs: this being removed, he fills the baſket with ſand a ſecond time; and thus continues ſifting for ſome time by the means of water.

The corpuſcles carried off by the water may be diſtinguiſhed, with regard to their bulk and weight into three kinds; firſt, the earth, duſt and every thing fine and light, is carried by the water to the bottom of the board: ſecondly, the groſſer matters being impelled both by the water and by their own weight, frequently likewiſe go as far as the bottom of the board; but the grains of gold are ſo fine, that there is no danger of their being mixed among theſe: thirdly, the fine, yet heavy grains, which could not be moiſtened and ſoftened like the duſt in their paſſage along the board, are detained by the nap of the cloth, which is to them like ſo many little dikes placed from ſpace to ſpace, which they are unable to ſurmount. It is among this laſt kind of matter that the gold duſt is found, which is here further blended with a bulk of ſand much greater than their own.

After filling the baſkets a certain number of times, the pieces of cloth are found covered over with ſand, and no longer in a condition to ſtop any more: upon this they take them off, and waſh them in a tub of water, to take off the ſand they have catched. Then tacking them on the board a ſecond time, they repeat the ſame operation a freſh, and thus continue to do till a certain quantity of this ſand have been gathered.

The method of waſhing hitherto has been groſs, and they proceed now to manage it with more precaution, putting ſome of the ſand in a wooden veſſel, hollowed like a boat. The waſher fills this boat with water, then taking it in both hands, he

he ſhakes it lightly, after a manner not unlike that uſed in ſifting corn, as in reality the end of thoſe two operations is much the ſame. The ſifter's deſign is to bring the chaff and light grains uppermoſt, and the intention of the waſher is alſo to bring the lighteſt ſand over the other, and give opportunity for the heavier grains to deſcend to the bottom of the veſſel; for the water in raiſing the light grains, and ſeparating from them the heavier, affords thoſe latter a means of diſengaging themſelves from the reſt, and ſlipping down. Laſtly, after a quantity of the lighter grains has got a-top, they pour the water gently off, which carries them with it. For the reſt it is eaſy to ſee whether it be light grains that are a-top, their colour being different from that of the reſt, and generally whitiſh. When the veſſel is ſet in an inclined ſituation, there are three rows or *ſtrata* of different matter, diſtinguiſhable between the top and bottom, being there ranged in the order of the ſpecific gravities.

This ſimple operation requires a great deal of patience and addreſs. They who make a practice of eſſaying ores are thoroughly acquainted with it, it being after this manner that they ſeparate the metalline parts or *ramentum* from the earth and ſand.

In the courſe of this ſifting of the water, they continue to take away the light white ſand, till what remains appearing of a deeper colour, ſparkles of gold begin to ſhew themſelves therein. In the ſand of the rivers *Ceze*, *Ariege*, and *Gardon*, they ſometimes on this occaſion meet with pieces big enough to be taken out by the hand.

In fine, when after ſeveral repeated lotions the ſand which comes a-top appears but little different

from what remains underneath, or when there is but very little of it, they ceafe this operation, and the fand is in the condition it fhould be for picking out the duft.

Notwithftanding all the ufelefs matter that has been taken away, what remains is ftill infinitely greater than the quantity of duft, which is but juft perceivable, here and there difperfed among the reft, efpecially when it happens to be fmall, as is that of the *Rhine* and *Rhone*. The feparation however is compleated by another lotion, as I may call it, with mercury. The fand being dried and heated, mercury is poured thereon, which being kneaded together with the hand, that no interftice may remain between the grains of fand, the mercury lays hold of what gold it meets with, and leaves the ufelefs matter alone. This matter is eafily afterwards feparated from the mercury by lotions in water.

The method of procuring the gold from the mercury mixed with it, is fufficiently known: they put it in a piece of fhammy fkin, and preffing it clofe, oblige the mercury to pafs through the fkin, leaving the gold alone behind. What little mercury ftill remains of the gold, is made to evaporate by placing it in a crucible over the fire.

Thus much for the method of procuring gold from the *Rhine*; for the other rivers we need only fpecify wherein the practice differs from this. —The fand wafhers of the *Rhone* make ufe of a board or plank like the former; but in lieu of faftening pieces of cloth thereon, cut grooves or furrows acrofs it about 4 inches afunder, two lines deep and four broad. In thefe furrows the fand fticks, as before in the nap of the cloth.

The fand-wafhers of the rivers *Ceze* and *Gardon* fpread a covering over the plank, fome of

goat's

goat's ſkin, others of hair, and others of wool. The grains of gold in theſe rivers being larger than thoſe of the *Rhine*, require proportionably higher and ſtronger dikes.

In ſome parts of theſe two laſt rivers the peaſants watch the time of their ſwelling, and when it comes cover their mill-dams with ſheep-ſkins. The waters coming to overflow, depoſite their gold-duſt thereon. Theſe ſkins, though leſs rich than thoſe of the *Argonauts*, might yet, like theirs, have become golden fleeces, if the poets had ſo pleaſed.

The ſand-waſhers of the *Ariege* do not uſe the inclined planks, but begin and end their lotions in a kind of ſhallow wooden platters, which they fill with water, or even work and agitate in the river itſelf; there is no harveſt without ſome loſs of the matter, which is its object; of which we have an inſtance in the gold-duſt. If it be not waſhed with the utmoſt care, the ſmaller grains will be carried off with the ſand; and they are ſometimes ſo very thin, that all the addreſs of the workmen is not ſufficient to ſtay them; gold leaf, every body knows, will ſwim on the water, and experience has inſtructed me, that if they be immerged to the bottom, they will riſe to the ſurface with a velocity equal to that of the lighteſt bodies; and the calx of the gold duſt of the *Ariege* affords us another inſtance of gold ſwimming in water. The natural grains of gold duſt may not perhaps be ſo thin either as the gold-beater's leaves, or as the grains of the gold calx of the *Ariege*, and yet be thin enough to give way too eaſily to the motions of the water.

Nor is it certain that the waſhers get all the gold out of the ſand which they have retained therein; they pour their mercury on it with as

little

little precaution as we ſhould pour it on water gold to make an amalgama, and yet there are ſeveral circumſtances wherein mercury will not act upon gold: thus a little fat or greaſineſs on the ſurface of the gold, is enough to hinder its effect. The *Indians*, who work moſt of their mines with mercury, take otherguiſe pains to make it take up the metal, and yet notwithſtanding all they can do, the gold and ſilver ſometimes gets off. Yet numerous inſtances of this kind related by *Alphonſo Barba*, who may be ſaid to be for the *Indian* mines, what *Agricola* is for the *German* ones; he even mentions ſome caſes wherein the mercury turns, as he calls it, into water; that is, becomes ſo attenuated and divided that it is loſt itſelf. One would ſuſpect that the *Indians* themſelves loſe a good deal of gold by being too ſparing of their mercury. M. *Frezier* relates that they ſometimes put 10, ſometimes 15, and ſometimes 20 *lb.* of mercury to $\frac{1}{2}$ a *caxon*, or 25 quintals of matter. Now I have put mercury in the greateſt of theſe proportions, which is as 1 to 125, upon the ſand of the river *Gardon*, and to facilitate the amalgama added ſalt and vinegar, grinding the whole together till the grains were no longer viſible: and, laſtly, to ſupply the defect of the hot air of *India*, to which the *caxons* lie expoſed near the ſpace of three weeks, I heated the mixture with fire; the effect was that the mercury did take up the gold, but did not take it all, for after ſeparating the ſand from it, we caſt a double doſe of mercury upon the ſame ſand, which brought away almoſt as much gold as the former.

The waſhers, it is to be obſerved, rarely undertake a large quantity of ſand, without firſt examining what they may expect from it; they begin

begin by eſſays, as all thoſe do who undertake to work mines, finding pretty nearly at what rate their time will be paid for, by the quantity of gold which they get from ſeveral little parcels of the ſands taken in different parts of the ſtrand. Upon theſe eſſays they are led to waſh the ſand in that part rather than the neighbouring ones.

All the gold-duſts we have obſerved, are very irregularly figured, and yet have this in common, that the grains are all ſo many little plates; I mean, that they are not to be conſidered as ſhaped like grains of ſand, but are leſs in thickneſs than in any other way. It appears as if they had been diſpoſed in layers or leaves in the ore; and they are ſometimes found palpably foliated when viewed with a magnifier. They are not however to be conceived with the thinneſs of leaf-gold, being thick enough for that dimenſion to be perceiveable, and withal to give them a ſufficient ſolidity. Their figures, notwithſtanding their irregularity, always approach towards a round; ſo that they may be conſidered as a kind of little cakes; their corners in effect have neceſſarily been broke off in their friction againſt the ſand, while driven along by the water.

Among thoſe of the rivers *Ceze* and *Gardon* we frequently meet with ſome a line and ½ in diameter, though there are more of thoſe not above a line, or even half a line. In the *Ariege* there are ſome full two lines the longeſt way; thoſe of the *Rhine* are much ſmaller, and thoſe of the *Rhone* ſmaller even than theſe: but I have always found the figure much the ſame in the ſmalleſt as in the larger.

It is affirmed however that grains have ſometimes been gathered in the *Rhone* as large as millet ſeeds, or even as vetches; and the *Germans* alledge

alledge ſome taken out of their rivers as big as beans; but theſe are but mites compared to thoſe huge pieces of gold found in *Peru* and *Mexico*, though inlarged perhaps in the recital. Father *Feuillee*, whom we may credit, aſſures us he has ſeen a *pepit*, which is the name they give theſe extraordinary pieces, in the cabinet of *Antonio Portocarero*, which weighed 66 marks and ſome ounces; and one was ſhewed the academy in 1716, which weighed, as we have been aſſured, 56 marks; it was in figure of a heart, and belonged to *Don Juan de Mur*, formerly corregidor of *Arica*. M. *Frezier*, who makes mention of this *pepit* in his voyages, quotes another bought by the count *de la Moncloa*, viceroy of *Peru*, to be preſented to the king of *Spain*, which weighed 64 marks. But theſe *pepits* ſeem as extraordinary to the inhabitants of the *Indies* as to us. They are properly intire pieces of ore detatched or left bare by rapid torrents; and we know not how large the pieces of gold may be which have ſo long furniſhed our rivers with duſt. It is probable we ſhould have *pepits* at home, if ſome extraordinary torrent or ſudden inundation ſhould tear away at once what is only carried off piecemeal in a number of years.

In ſome places the river gold grows to fragments of ſtones, of which we have inſtances in *Fabritius*; the reaſon is, that the veins of ore are only a kind of ſlender threads which are cloſely united to the ſtone; ſo that the ſame impulſe which carries off the leaf of gold, carries off the ſtone it ſticks to. But we have all the probability that the gold is in large maſſes, in ſuch places where it is detached in pure grains.

The ſand wherewith theſe grains are mixed, is itſelf a kind of treaſure to the obſervers of nature.

Though

Though the washers throw it away as useless, we may distinguish three kinds thereof, *viz.* white, red, and black.—The white sand carried off by the first lotions, when viewed by the microscope, appears composed of crystals like those of common sand, and accordingly may be reckoned as such.

The reddish sand when viewed by a microscope, or even a magnifier, affords the finest spectacle in the world, being an assemblage of all the transparent and coloured stones known among jewellers; it is nothing but rubies, saphires, emeralds, hyacinths, *&c.* The stones most common in it are of the several shades, from the colour of a balas ruby, to that of a hyacinth, and hence the redness of this sand to the naked eye. Saphires, topasses, and emeralds are more rare, and yet we find very beautiful ones among the rest.

As to the black sand, it is almost all iron, and accordingly affected by the loadstone, as if it were iron filings itself. This sand is much more copious among that of the *Rhone* than that of the *Rhine*. I have drawn from the former, by a knife touched with the loadstone almost $\frac{1}{3}$ of the quantity in iron, which makes me imagine that the washers might make good use of such knives to disengage the gold from a great part of the useless matter: this would be more expeditious than by the last lotions; but then care must be taken to dry the sand ere it be presented to the knife, otherwise the rust would spoil it.

There is some of the black sand however upon which the knife will not act, and which probably is not iron, since though exposed sometime to the fire, it becomes no more attractable, and yet the weight of this sand seems to prove it metalline;

but there being but little of this mixed among a great deal of the red ſand, it is not eaſy to make a trial.

The red ſand, or that conſiſting of a heap of grains of lively but different colours, is alſo of a weight approaching that of a metalline ſand, as being incapable by lotion of being ſeparated from grains of iron, though the ſize of ſuch grains be but little bigger than their own; whence we infer that this alſo contains metalline parts.

Veins of ore are commonly ſurrounded with tranſparent ſtones like cryſtals, only ſofter; they are called fluors becauſe they melt eaſily by the fire, and are uſed in fuzing the ore; theſe two are of different colours, and there may be room to take our grains of ſand for fragments of ſuch ſtones.

I am inclined however to conſider theſe grains of ſand as ſo many coloured ſtones of ſuch hardneſs as this kingdom is able to furniſh, and that upon the following obſervation. In the *Puy en Velay* we meet with ſtones of different colours, which are of ſome value among jewellers, when ſufficiently big. They are gathered in a rivulet called *Peroulliou*, and ſeparated from the ſand by lotion: the ſand mixed with them only differs from that found among the gold duſt, as gravel differs from common ſand. I have ſeen ſtones of all colours among this ſand, but hyacinths prevail the moſt, as they alſo do among our ſand. In fine, what compleats the reſemblance is, that theſe ſtones are intermixed with a black ſand, or which was ſent us for ſuch, and which upon our examining it with a magnetical knife, was readily attracted thereby.

The grains of the ſand found in the *Rhine* are of deeper colours than thoſe of the *Rhone*. Theſe

latter

latter are frequently only a very ſlight tincture of carnation, like a pale balas ruby; and yet there are topaſſes, emeralds, *&c.* found among them.

There is no need to note why we have laid down black ſand, and ſand of a different colour from the common ſand, as indications of the moſt advantageous places for ſeeking the duſt. It is not that they have any immediate relation to each other, but if the river have ſwept away any duſt, it muſt let them ſettle among the other heavy matters. But beſide the three ſpecies of ſand above mentioned, there is a fourth found in ſome rivers, which has ſometimes puffed the duſt ſeekers up with a vain expectation. This ſand is common in the river *Gardon*, being only an aſſemblage of talky grains, with all the colour and luſtre of the fineſt gold; and inſtead of loſing this colour by the fire, it raiſes and improves it. There was a mine found ſome years ago at *Vigean* towards the upper *Poitou*, which made a good deal of noiſe upon the appearance of theſe fallacious grains; the matter of the ore was very black and hard, but when expoſed to the fire became intermixed with gold ſparkles, at leaſt they were taken for ſuch, the fire giving a gold colour to the talky particles found therein.

Yet the talky grains may be ſafely diſtinguiſhed from the golden ones, without making any eſſay; they need only be viewed with a magnifier. The corners and edges of the gold duſt we have obſerved are always rounded, which is never found in the talky ones; friction may break them, but can never ſmooth or poliſh them; ſo their edges ſtill remain ſharp or uneven, a property which always attends talcs.

'Tis remarkable that tho' gold be the rareſt of all metals, yet the rivers which yield duſt thereof,

are more numerous than thoſe of any other metal, excepting iron, which is ſo plentiful in *Europe*, and eſpecially in this kingdom, that there is no place without it. There are few rivers, for inſtance, which carry ſilver-duſt, as has been well obſerved by *Charleton* in his *Onomaſticon*. The ſame is remarked by *Fabricius*, who adds, that there are none in *Germany*; and ſome metalurgiſts make a doubt whether there be any at all. So likewiſe there are but few rivers which yield either lead, tin, or copper pure.

But though it be granted that gold is more frequently found pure in its mine than moſt other metals, the reaſon hereof will ſtill remain to be explained. All we can add is, that when once formed it may maintain itſelf along while in any place where it may chance to be carried, as not being ſubject to ſo many alterations as other metals; add, that being ſofter than either ſilver or copper, it is eaſier to be ſeparated by the current of rivers.

When we ſay that gold is found pure in the mines, we only mean that it appears ſuch, and is already malleable; not but there may be other metalline or mineral particles mixed along with it; art itſelf can hardly purify it to ſuch a degree, either by the depart or by antimony; it is almoſt always alloyed either with copper or ſilver, and commonly with both. We have eſſayed the gold of ſuch of our rivers as we could get a ſufficient quantity of, and find that the gold of the river *Ceze* is 18 caracts 8 grains fine; ſo that before it be refined, it contains near $\frac{1}{4}$ of its weight in copper or ſilver. That of the *Rhone* contains only a 6th part of thoſe foreign matters, being 20 caracts fine; that of the *Rhine* is 21 caracts and

and $\frac{1}{4}$, and that of the *Ariege* being the pureſt of all others 22 caracts and $\frac{1}{4}$.

We do not pretend by theſe eſſays to fix ſtandards for the gold of thoſe different rivers, all that can be learned from them is that one yields a purer gold than another, for the degree of fineneſs is variable even in the ſame piece of gold; the large *pepit* ſhown the academy was 23 caracts $\frac{1}{2}$ in one place, 23 in another, and 22 in another; that mentioned by father *Feuillee* was 22 caracts 2 grains, in its upper part, 21 caracts $\frac{1}{2}$ a grain a little lower, and within 2 inches of the bottom only 17 $\frac{1}{2}$; nor do I ſee why ſome authors ſhould be ſo perplexed to account for this diverſity; it is a common caſe for a piece of ore to be richer in one place than another, the like of which alſo holds in the mine itſelf, and the wonder would be if it were otherwiſe.

We have alſo eſſayed ſeveral grains of the ſame rivers, with as much exactneſs as their little bulk would admit of, that is by the touchſtone alone. An eſſay of this kind is not enough to determine the preciſe fineneſs, but may ſuffice to ſhew that different grains of the ſame river duſt are different therein.

IV. *An account of an extraordinary kind of iron ore found in the* Pays de Foix; *with ſome reflections on the manner wherein it was formed*; *by M.* de Reaumur; *tranſlated by Mr.* Chambers*.

In a former memoir upon iron ores, I have noted the principal diverſities of figure, ſtructure, and colour which occur therein, and after diſtri-

* June 15, 1718.

buting

buting them with regard to thefe diverfities into *genera* and fpecies; I fubjoined fomething concerning their formation, advancing that from what I had obferved of their ftructure both internal and external; it clearly appeared, 1ft, that the production of iron ores like that of ftone is continued every day. 2dly, that the ore newly produced owes its formation to iron diffolved in fome fluid, for inftance in water, or in water impregnated with fome other diffolvent, and that this iron was detained in ftones, clods of earth, or other matter, which hereupon became iron ore. I further added that to account for the formation of the grains, or round bits of ore, which like bezoards or onions confifts of feveral coats or *ftrata*, we muft fuppofe that fome drops of the fluid impregnated with iron had fallen from the top of the vault or cavern to the bottom, and there formed the grains of ore, as the drops of water impregnated with ftony matter form thofe little round prominences found in grottos under-ground. In a word, as the common caverns only diftil a water impregnated with a meer ftony or cryftal matter, I fuppos'd that there are other caverns where the water diftilling from them contained a ferruginous matter, and that whereas the concretions of the former caverns were ftones or *ftalactites* or cryftals of diverfe kinds, the concretions of the latter were iron ore.

This reafoning though chiefly founded on analogy had a great fhare of probability; it is allowable even in phyficks to advance conjectures, provided we only confider them as fuch, until we we have found out proofs which deferve another name; and fome pieces of iron ore fent to the duke of *Orleans* from the *Pays de Foix*, feem happily to demonftrate the truth of what I have advanced in the prefent inftance.

Some

Some of the pieces from the mine of *Gudannes*, are ſo very ſingular, that at firſt ſight one would take them for the works of art, and every body I ſhowed them to took them accordingly for ſuch. One would imagine that they had been covered over with a coat of the blackeſt amel, the cruſt they are ſurrounded with only differing from amel in this, that it is harder, ſmoother, and more gloſſy; its hardneſs is equal to that of cryſtal, and its colour to that of the fineſt jet, and yet it is certain this coat is the work of nature, for beſides that art cannot come up to ſuch a hardneſs, we find all the inſide, to the very centre, poſſeſſed by a matter in all reſpects like that of a common ore, and the *radii* of the external coat are all directed to this ſubſtance ;but what is moſt conſiderable in the outſide of the ore, at leaſt to a naturaliſt, is the inequalities which to another perſon would perhaps ſeem a disfigurement ; theſe inequalities riſe prominent being they are broader and thicker at one end than the other, like what we call *ſtalactites*, or thoſe congelations produced by a dripping liquor, their figure is a proof of this, and this proof is rendered compleat by their direction, they having all of them the ſame direction as heavy bodies have, which deſcend freely.

If theſe pieces afford us inſtances of iron ore formed like the congelations ſticking on the vaults of caverns, the ſame mine furniſhes other pieces, which if viſible had been formed like the congelations at the bottom of caverns, the ſtructure and arrangement is the ſame in thoſe congelations and our ores, the coats are in ſome meaſure waved, and compoſed of drops fallen one upon another.

But it is obſervable that theſe latter pieces of ore are inferior in luſtre to the former, their colour having

having been altered by the mixture of ſome other ſofter and leſs tranſparent matter.

If it be enquired what the matter is which gives the firſt kind of ore its fine coat, there will be no great difficulty in finding it, the place where it is made being the ſame wherein cryſtals are formed, leads us to imagine, that whereas the common ores have an earthy matter for their baſe, this has a cryſtalline one, which cryſtal being penetrated by the iron, forms a natural black amel, as art likewiſe uſes iron to make the amel of this colour. M. *Lemery* has proved that there is no matter ſo fit as iron to afford a black colour, it being to this that ink owes its hew; if any further proof were required, that it is the cryſtalline matter makes the baſe of a black cruſt, it might be had, from ſome pieces of ore furniſhed from the ſame mine; in ſome of theſe pieces I have found actual cryſtallizations ſtill white and tranſparent, as having not yet been mixed with any of the ferruginous matter nor tinged therewith; nor need it be apprehended that this cruſt ſhould prove leſs rich in metal, becauſe cryſtal is its baſe. I have elſewhere found ores of the like colour, for inſtance thoſe mixed among the ſtones *du Pui en Velay*, the grains whereof would yet be attracted by an animated knife, like pure iron itſelf.

V. *Of the magnitude and figure of the earth; by M.* Caſſini*.

There is not any thing that appears more worthy of our inquiries, than to know the magnitude and figure of the earth, nor does any thing appear more difficult to be undertaken.

* November 12 1718.

For

For how ſhall we meaſure this vaſt extent of continents, the ſurface of which is covered with an infinite number of mountains which render it unequal, and is interſected ſo many ways by rivers, lakes and ſeas, which alſo ſurround it on all ſides. Thus *Pliny* wondered at the boldneſs of the human mind in daring to attempt things ſo difficult, and it could never have been poſſible to ſucceed therein if we had not attempted to determine the whole circuit of the earth, by the meaſure of one of its parts, ſuppoſing its figure ſpherical, and the degrees of its circumference equal between themſelves.

This *hypotheſis* of the roundneſs of the earth was not at firſt generally received by all the philoſophers, ſome of whom had very ſingular opinions on this ſubject. It was afterwards eſtabliſhed by the appearance of the ſhadow of the earth in the eclipſes of the moon, which always appears circular, and principally by the different conſtellations which are perceived in the ſouthern and northern countries, and are ſeen to riſe or ſet according as we travel either to N. or S.

Theſe different appearances are related at large by *Ariſtotle* in his ſecond book *de cœlo*, where he adds, that in his time the mathematicians had determined the circumference of the earth to be 400,000 *ſtadia*, or 20,833 leagues of 2000 toiſes each, ſuch as they are about *Paris*.

He does not ſhew the methods which were uſed for this inquiry, which are reduced to two principal ones. The firſt by the obſervation of the ſtars which paſs through the vertical of one place, and are diſtant from the vertical of another. The ſecond, by the obſervation of the ſtars, which at their paſſage through the meridian appear at the

horizon of one place, and are at the ſame time elevated above the horizon of another.

The firſt method was practiſed under king *Ptolomy Euergetes*, by *Eratoſthenes*, between *Alexandria* and *Sienna*, diſtant from one another 5000 *ſtadia*; the ſecond in the time of *Pompey* by *Poſſidonius*, between *Alexandria* and *Rhodes*, which he ſuppoſed to be at the ſame diſtance from each other.

The firſt of theſe dimenſions makes the degree $694\frac{1}{2}$ *ſtadia*, and the circumference of the earth 250,000 *ſtadia*, which *Eratoſthenes* afterwards ſuppoſed to be 252,000 *ſtadia*, to have the round number of 700 *ſtadia* in a degree, that is, 72,917 toiſes, each of 6 feet, or 36 leagues, each of 2000 toiſes.

The ſecond, related by *Cleomedes*, determines this circumference to be 240,000 *ſtadia*; but *Strabo*, who wrote his geography under *Auguſtus*, aſcribes to *Poſſidonius* the dimenſion of the circumference of the earth of 180,000 *ſtadia*, leſs by $\frac{1}{4}$ than the preceding, which probably ariſes from the diſtance between *Alexandria* and *Sienna*, which was ſuppoſed by *Poſſidonius* to be 5000 *ſtadia*, having been, as *Strabo* relates, meaſured by *Eratoſthenes* with inſtruments 3750 *ſtadia*.

This dimenſion, according to which the magnitude of a degree would be but 52083 toiſes, was received by *Marinus Tyrius*, and other geographers, and is commonly aſcribed to *Ptolomy*, becauſe he employed it in his geography.

After *Eratoſthenes* and *Poſſidonius* the mathematicians of the caliph *Almamon*, having obſerved in the plains of *Singar*, the height of the pole of two places, diſtant from one another 2° from S. to N. found 56 miles in one of theſe degrees, and $56\frac{2}{3}$ miles in the other, which makes the magnitude of the

the degree lefs than the other dimenfions, which had been made before.

From that time to the laft century, we do not know of any other attempts made on this fubject except that of *Fernelius*, a celebrated phyfician and mathematician, who having obferved from *Paris* toward *Amiens*, the height of the pole of two places, 1 degree diftant from one another, from S. to N. meafured their diftance by the revolutions of coach wheels, and having abated the turns at difcretion, determined the magnitude to be 56,746 toifes.

After him, *Snellius*, a *Dutch* mathematician, meafured geometrically the diftance between *Alkmaer* and *Bergen op zoom*, and having obferved the height of the pole of thefe two cities, which he found to be diftant from one another $1^{\circ}\ 11'\frac{1}{2}$, he eftablifhed the magnitude of a degree of the circumference of the earth to be 57,000 *Rhinland* toifes, which being reduced to ours, make 55,021 *Paris* toifes.

The method ufed by him in this enquiry is exact, but the errors which have flipped into this obfervation, or elfe into the calculation and the impreffion, render his meafure uncertain, as I have obferved in the memoirs of 1701.

Some time after F. *Riccioli* undertook at *Bologna* to determine the magnitude of the earth, which he executed different ways, whence he concluded the magnitude of the degree to be 64,363 *Bolonian* paces, or 62650 *Paris* toifes. Thus in the laft century they were ftill in doubt about the magnitude of the degree 7642 toifes, and about the circumference of the earth 1375 leagues, which make about the 7th, or 8th part, and it feemed that the exact determination of the magnitude of the earth was referved for the royal academy of fciences

and our nation, which under the preceding reign, as well as under the present government, has rendered itself at least as famous by the profession of arts and sciences, as by that of arms.

In short, one of the first objects of this academy, soon after its establishment, was to labour about what might contribute to the perfection of geography and navigation, sciences, as all the world knows, of the greatest use to civil society, and to commerce, which causes states to flourish.

Now the measure of the earth is the *basis* and foundation of these two sciences. For having determined by astronomical observations the longitude and latitude of several places of the earth, it is necessary to know the magnitude of the degrees, to be able to reduce them to a certain measure, and to have the true distance between these places; and reciprocally knowing the true distances between several places of the earth by geometrical operations, or itinerary measures, it is necessary, if we would reduce them into degrees, to know exactly the true value of them.

It was in order to know this proportion, that M. *Picard* undertook the celebrated measure of the earth, which surpasses in exactness all that had before been undertaken on the same subject.

With this design he measured the space between the parallels of *Malvoisine*, *Sourdon*, and *Amiens*, which contains about a degree and $\frac{1}{3}$ of the circumference of the earth; and having observed in these different places, with an instrument of 10 feet *radius*, the distance from the zenith of a star which was not far from it, he determined the magnitude of a degree of a meridian to be 57,060 toises, which he judges to be not very far from the true measure of the degree; though he adds, that we may still arrive to a

greater

greater degree of exactneſs, by meaſuring with the ſame care and like inſtruments a much greater diſtance than that of *Malvoiſine* and *Amiens*. The moſt exact of his meaſures ended, as he himſelf confeſſes, at *Sourdon*, which is five or ſix leagues ſhort of *Amiens*, and contained but a little above a degree of the circumference of the earth, which was very little with regard to its whole compaſs. They alſo ſuppoſed the earth to be ſpherical; that all the degrees of the ſame meridian were equal, and that the lines perpendicular to the horizon, which meaſure the degrees in the heaven, were all directed to the centre of the earth.

But however theſe *hypotheſes* were not generally received by all the modern philoſophers.

Some having obſerved, that in the dimenſions made at different diſtances from the pole, the magnitude of the degree had been found ſmaller towards the poles than towards the equator, judged that the degrees of the ſame meridian diminiſhed in proportion as they receded from the equator; which renders the earth of an elliptical figure, prolonged towards the poles. Others having conſidered that the vibrations of the pendulums were of different lengths under different climates, conjectured according to the principles of the force, which they call centrifugal, that the earth was flatted towards the poles, and that its degrees increaſed in proportion as they drew near the poles.

This inquiry, more uſeful in practice than curious in ſpeculation, deſerved well to be examined with all poſſible exactneſs, by meaſuring upon the ſame meridian the greateſt diſtance that could be found, and obſerving in different places of this meaſure the correſponding arches of this meridian.

This

This was the subject of a memoir presented in 1683 to M. *Colbert* by my father, in which he proposed to measure from S. to N. the whole extent of *France*, which contains above 8 ½ degrees of the circumference of the earth.

This proposal was agreed to by the king, who gave orders to execute it. For this measure the meridian was chosen, which passes through the middle of the royal observatory at *Paris*, which, by the observations that have been either made or compared there, is become without contradiction the most celebrated of all the known meridians of the whole earth.

My father went toward the southern part of the kingdom, and M. *de la Hire* toward the northern part. But this work was interrupted soon after, by the death of M. *Colbert*, and by the wars which succeeded, till 1700; when the king having been informed by the count *de Pontchartrain*, and the abbé *Bignon*, of the advantages that might be drawn from the continuation of this work, gave orders to my father to resume it at the place where it had been interrupted.

We have already given an account to the publick of the success of this work, in which I was employed with Mess^rs^. *Maraldi*, *Couplet*, and *Chazelles*. The magnitude of each degree was there determined to be, one with another, 57097 toises, a little greater than it had been found by M. *Picard*; which gave us room to conjecture, that the degrees of a meridian increase as they approach the equator; whence it results, that the earth is prolonged towards the poles, as I have demonstrated in the memoirs of the academy for 1710.

But this difference being but 11 or 12 toises in a degree in the extent of *France*, seemed too little

for

for us to be certain that it was not caufed by fmall errors, almoft infeparable from the obfervations. Thus the meridian line having been defcribed quite to the fouthern extremity of *France*, it was neceffary to prolong it to the northern extremity, to know whether this diminution of the degrees towards the poles was conftant.

This was the object of our laft journey; the ufefulnefs of which having been reprefented by the *Abbé Bignon* to his royal highnefs the duke of *Orleans*, regent of the kingdom, I received orders to execute it in concert with Meff. *Maraldi* and *de la Hire* the fon, who had lately fucceeded his father, the lofs of whom can never be too much lamented. That we might neglect nothing that could tend to its exactnefs, we thought it proper to begin our operations at the places where M. *Picard* had left off his moft exact meafures.

We went for this purpofe to *Montdidier*, whence we obferved, on the 2d of laft *June*, the angles of pofition, with the diftant objects which were thereabouts, which we continued to do in the places vifible from one another, to form a *feries* of uninterrupted triangles quite to the northern extremity of the kingdom.

We fhall afterwards give the publick the particulars of the operations which were made in the courfe of this journey: it will be fufficient to obferve at prefent, that we carried our quadrants into the towers or fteeples which feemed the moft advantageoufly fituated for the continuation of the triangles; and as in *Picardy*, *Artois*, and *Flanders*, moft of thefe fteeples are furrounded by very high trees, we were obliged to place ourfelves as high as poffible, to erect fcaffolds, and to cut through them in different places, to difcover the moft diftant objects round about.

We

We thus continued our obſervations to three or four leagues on this ſide of *Bethune*, where there is a chain of mountains extending from E to W. upon which there is no remarkable object; wherefore having ſought for the place that is beſt expoſed, we prepared a ſignal that could be perceived both on the northern and ſouthern ſide, and might ſerve us to continue the ſeries of our triangles.

It is from theſe mountains that we began to perceive *Mont-Caſſel*, and ſeveral towns or remarkable objects, which led us quite to the ſea.

By this means we have from the obſervatory at *Paris* to *Dunkirk* 28 triangles, 9 old ones obſerved by M. *Picard*, and 19 new ones, which contain all the northern part of the kingdom that is about the meridian.

In the multiplicity of theſe operations, there was danger of ſome error having ſlipped in. To avoid them, or diſcover them, we have verified our principal triangles by a number of others, which were terminated by other objects, which we have looked upon as acceſſory, but they gave us almoſt the ſame diſtances. Beſides this advantage, we had that of determining by the ſame means the ſituation of a great number of remarkable places, and of the greateſt part of the towns of *Artois* and *Flanders*, which is of very great uſe in making or rectifying the particular maps of this country, which is uſually the ſeat of war, and is neceſſary to be known exactly.

Laſtly, that we might omit nothing that regards the exactneſs of this work, we terminated our laſt triangle by a baſe of 5564 toiſes, which has been twice meaſured exactly upon the ſea-ſhore, with a difference between them only of three feet, which may be aſcribed to the ſands,

which the sea by its flux and reflux continually transports from one place to another, which causes some little inequalities in the shore.

This actual measure is found to agree within about a toise with that which resulted from the *series* of the triangles, calculated upon a base almost of the same length measured by M. *Picard* between *Villejuisve* and *Juvisy*, which had served for a foundation to his measure of the earth; which is a proof of the exactness of the observations, which have been made to form these triangles.

To determine the places through which the meridian line passes, we first observed the declination of the objects employed by M. *Picard* in his measure, with regard to the meridian which passes through the middle of the observatory. We had by this means successively the declination of the other objects with regard to this meridian, and have calculated the distance of each of these objects from the parallel, and from the meridian of *Paris*.

We have found by this method the distance of the middle of the southern face of the observatory from the parallel which passes through the centre of the great tower of *Dunkirk* to be 125,552 toises, and the distance of this tower from the meridian 1414 toises; by which *Dunkirk* is more to the E. This difference being reduced into minutes of a degree of a parallel, gives the difference of longitude between *Paris* and *Dunkirk* 2′ 20″ of a degree, or 9″ $\frac{1}{3}$ of an hour, at the rate of 15° to an hour. This difference agrees to a pretty considerable exactness with what had been determined 37 years before by the observations of the satellites of *Jupiter*, made at *Dunkirk* by M. *de la Hire*, compared with those which my father

 had

had made at the ſame time at *Paris*. Two of theſe obſervations which are related in the journies of the academy printed in 1693, give this difference, one of 3″ and the other of 8″, within about a ſecond of what reſults from our dimenſions. Thus we ſee the agreement of theſe two methods in the determination of the longitudes, and the exactneſs that may be hoped therefrom.

The diſtance between the parallels of *Paris* and *Dunkirk* being determined in toiſes, we wanted to know the arch of the meridian intercepted between theſe two cities, to have the juſt meaſure of the degrees contained in this ſpace. We had carried for this purpoſe an inſtrument of near 10 feet *radius*, the limb of which was divided into 12 degrees, and each degree at every 20 ſeconds, ſo that we could eaſily diſtinguiſh to 5 ſeconds.

To place this inſtrument, we cauſed to be built a little obſervatory open at the top, to determine the diſtances of ſome fixed ſtars from the zenith, which we obſerved by directing the teleſcope to the ſame ſtar ſucceſſively, on the ſouth and on the north ſide, with regard to the inſtrument, to have upon the limb two points equally diſtant from the zenith, and to determine their true diſtance exactly. We made uſe in theſe obſervations of ſeveral ſtars of the *dragon* and of the *ſwan*, and we continued them till we had ſeveral exact ones, which gave exactly the ſame height.

The magnitude of the inſtrument uſed in theſe obſervations, renders them very difficult, and the precautions that muſt be taken to make them exactly, have given us occaſion to make on this ſubject ſome ſingular remarks, very uſeful in the practiſe of aſtronomy, which we ſhall preſent to the publick hereafter.

The

The obſervation of the ſame ſtars having been made at the obſervatory with the ſame inſtrument preſently after our return, at the end of *Auguſt* and beginning of *September*, we found the arch of the meridian between *Paris* and *Dunkirk* 2° 12′ 15″. Comparing it with the interval which we have juſt determined between theſe two places of 125,552 toiſes, we have the magnitude of each degree, one with another, 56,960 toiſes, leſs by 90 toiſes than according to M. *Picard*, and by 137 toiſes than that which we had determined from the obſervatory to the ſouthern part of the kingdom.

Thus it plainly appears, that the degrees of the ſame meridian are not all equal between themſelves, that they diminiſh as they approach the pole, and increaſe as they recede from it; and we ſhall be the more eaſily convinced of this, if we conſider, that the diſtance from the zenith of the ſtar in the dragon's head marked γ by *Bayer*, which we made uſe of principally for this inquiry, has been determined the ſame quantity ſeven times at *Paris* without any difference; we have alſo carried our exactneſs ſo far as to verify, with great care, and in different manners, the diviſions of our inſtrument, that we may leave no ſcruple on that ſide.

Laſtly, to verify the direction of the meridian of *Paris*, we have obſerved from the top of the tower of *Dunkirk*, the place where the ſun ſet, and the point of the horizon which anſwered to his paſſage over the meridian, to determine immediately the ſituation of the meridian, which we have found within a ſmall trifle conformable to that which reſulted from the ſeries of the triangles.

It remains now to conſider what is the magnitude of the degrees which reſults from the obſer-

vations made in the northern part of *France*, compared with thoſe which had been made in the ſouthern.

We ſhall uſe for this inquiry, the method which we have given in the memoirs of the academy for 1713, to determine the inequality of the degrees, by ſuppoſing that the earth has the figure of an *ellipſis* flatted towards the poles, and we ſhall find that, ſuppoſing the magnitude of a degree about *Paris* to be 57,006 toiſes, and the dimunition from one degree to the other 31 toiſes, that which is between the 42d and 43d, towards the ſouthern extremity of the kingdom, is 57,192 toiſes; and from the 50th to the 51ſt, toward the northern extremity of *France*, 56,944, with a difference from the greateſt to the leaſt of 248 toiſes; and that the ſum of degrees of the meridian, contained between the parallels of *Collioure* in *Rouſſillon* and *Dunkirk*, is 486,156 toiſes, equal to that which has been meaſured by the trigonometrical operations.

We ſhall find alſo that the difference between the *foci* of the *ellipſis*, which repreſents the earth, is 947,434 toiſes, or 474 leagues of 2000 toiſes each; that the *axis* of the earth, or the diameter which paſſes through the poles, is 6,579,368 toiſes, or 3790 leagues, greater than the diameter of the equator by 68,572 toiſes, or about 34 of our leagues: that the firſt degree toward the equator is 58,020 toiſes, greater than that which ends at one of the poles by 1795 toiſes, or about $\frac{3}{4}$ of a league; and that the degrees of the equator, which, according to this hypotheſis, are all equal between themſelves, are each 56,817 toiſes.

Adding together the degrees of a meridian, we ſhall have the circumference of the earth from one pole to the other 20,563,100 toiſes, greater by

by 108,826 toiſes, or 54 of our leagues, than the circumference of the equator.

We ſhall know by this means, the moſt exactly that is poſſible, the magnitude of the earth and its figure; two parts of knowledge, as was explained before, ſo neceſſary for geography and navigation, of which they are the foundation and baſe.

VI. *An obſervation of a meteor, which appeared* Oct. 23, 1718, *by M.* de la Hire.

We obſerved, *Oct.* 23, at 11 at night, that there was from the N. E. to the W. a very thick cloud, which extended from the horizon to 7 or 8° in height; and that there came out from this cloud toward the N. E. a light of 7 or 8° in height, and about 2° in breadth. This light, which was almoſt as ſtrong as that of the ſtars, ran through the whole extent of the cloud, with a pretty quick motion, changing its ſhape as it went along.

We conſidered this cloud about an hour, and there came out of it during that time, in different places, above 8 or 10 of theſe lights, of different heights and breadths, and having all the ſame motion.

VII. *An examination of the cauſes of the impreſſion of plants marked on certain ſtones about* Saint-Chaumont *in the* Lionnois; *by* M. de Juſſieu *.

The country about *Saint-Chaumont*, as well as that about *Saint-Eſtienne*, is known to abound in coal mines. This convenience has occaſioned the

* Nov. 12, 1718.

eſtabliſhment

eſtabliſhment of the manufactures of all ſorts of iron works.

As I had remembered to have read in Mr. *Lhwyd*'s letters, that the ſtones imprinted with figures of plants, are moſt commonly found in the neighbourhood of coal mines, rendered me attentive to the figure, the colour, and the impreſſions of all the ſtones which I found near theſe mines. My attention was ſeconded by the ſight of the ſpecimens given me by one of my friends, who is diſtinguiſhed in the country by his taſte for natural hiſtory.

With this guide I had the pleaſure, at the very gate of *Saint-Chaumont*, along the little river of *Giés*, to obſerve upon moſt of the ſtones that I gathered, the impreſſions of an infinite number of fragments of plants, ſo different from all that grow in the *Lionnois*, in the neighbouring provinces, and even in the reſt of *France*, that I ſeemed to herborize in a new world.

All theſe ſtones are ſcaly, and differ among themſelves in colour, only ſo far as the beds whence they are taken approach to thoſe of the coal, or recede from it; that is, as thoſe which are neareſt are of a ſlaty and ſhining black, in which they ſeem to partake more of the bituminous oil, which is the moſt eſſential principle of this coal; whereas thoſe which are the moſt diſtant from it are of an aſh colour, which a mixture of talcy particles make appear ſometimes brazen, and moſt often ſilvered.

In both theſe ſtones, of what colour ſoever they are, the impreſſions are always deeper than the reſt, and they are very diſtinct from the grey ones: ſometimes alſo they are the only part that appears covered with a light *ſtratum*, of copper or ſilver; which is an effect of the facility that the

the vitriolic fluors had to ſtop in the furrows of theſe impreſſions, rather than in the reſt of the ſurface of theſe ſtones.

They are different from the *dendrites*, hard like agats or flints, or tender like whetſtones, and the ſtones of *Florence*; in this, that the figures found in theſe, penetrate the whole thickneſs of it like a foreign matter inſinuated into it, which has been very well obſerved by the late M. *de la Fay*; whereas in the ſtones of *Saint-Chaumont*, the impreſſions of plants are only on the ſurface of the *laminæ*, and in each of the *laminæ* that compoſe it, they are all different, and placed different ways.

They are generally capillary plants, ceterachs, polypodies, maidenhairs, harts-tongues, ſpleenworths, oſmunds, *filiculæ*, and ferns, approaching to thoſe which *F. Plumier* and Sir *Hans Sloane* have diſcovered in the *American* iſlands, and to thoſe which have been ſent from the *Eaſt* and *Weſt-Indies* to the *Engliſh*; and communicated to *Plukenet*, to enter into his collection of rare plants. One of the principal proofs that they are of this family is, that as they are the only ones that bear their fruits faſtened to the backs of their leaves; the deep impreſſions of their ſeeds are even diſtinguiſhed upon ſome of theſe ſtones.

The multitude of the differences of theſe plants is beſides ſo great about *Saint-Chaumont*, that each quarter ſeems to be a ſource of varieties.

Beſides theſe impreſſions of leaves of capillary plants, I have alſo obſerved ſome which ſeem to belong to palms and other exotic trees. I have alſo obſerved ſome particular ſtalks and ſeeds, and at the opening of ſome of the *laminæ* of theſe ſtones, there has come out of the vacuities of ſome of the furrows a black duſt, which was nothing but

but the remnants of the plant rotted and inclosed between two *strata*, perhaps for above three thousand years.

In this discovery there are three particulars which render it very remarkable.

The first is that we do not find in the country any of the species of plants, of which the impressions are found marked on these stones. This fact I was satisfied of in the herborisations which I made immediately afterwards on the neighbouring mountains, and principally upon that of *Pila* in *Lionnis*, which is but about three leagues from *St. Chaumont*.

The second is, that among this infinite number of leaves of different plants imprinted on the *lamine* of these stones, none is found bent, and that they are spread just as if they had been pasted on.

The third singularity, more surprising than the other two, is that the two scaly *lamina* of these stones represent each of them on their internal surfaces where they touch, only one face of a leaf of a plant in *relievo* on the one side, and hollow on the other; wheras in the common manner in which we conceive these sorts of impressions, we suppose that the leaf of a plant which is pressed between 2 soft pieces of earth, must have left upon the surface of one, the impression of its upper part, and on the surface of the other the impression of its lower part.

These singularities suppose several causes necessarily depending on each other.

The first, that these plants unknown in *Europe* can come only from the hot countries, because if they resemble more perfectly those of our *American* islands than any others, and if we find these same species of *American* plants which they resemble only

only in different parts of the *Indies* where they grow abundantly, it follows that they muſt have been brought either from thoſe countries, or from ſome other ſuch like ſituation.

The ſecond, that as their impreſſion repreſents them ſpread, and often laid different ways from one another, they were imprinted in this ſtate only becauſe the water, upon which they muſt have floated, has kept them up.

The third, that it was undoubtedly ſea water; which is evident from the number of ſhells that are found in the neighbouring lands; ſhells which we do not find in any of the freſh waters in *France* nor even in *Europe*; but on the contrary are found ſome on our coaſts, and others on the moſt diſtant ſeas with regard to us.

A daily experience of the viciſſitudes which happen to certain countries, the lands of which the ſea overflows or diſcovers ſucceſſively, does but too much ſhew us how theſe waters, which we ſuppoſe to have tranſported theſe plants, may have covered theſe parts of the *Lionnois*; for without being obliged to have recourſe either to the inundation of the univerſal deluge, or to thoſe earthquakes, or conſiderable ſhocks, which have made great apertures through which the water of the ſea has been ſpread, without ſpeaking of the frightful tumbling down of thoſe high and vaſt mountains, the fall of which having poſſeſſed a great ſpace in the channel of the ſea, has thrown up the water very far upon our lands; we are in no want of proofs, that moſt of the lands, which ſeem to have been inhabited from time immemorial, have been originally covered with the water of the ſea, which has ſince abandoned them either inſenſibly or all at once.

The multitude of ſea ſhells which are ſtill found entire almoſt in the centre of the mountains of *Si-*

cily and *England*, does not permit us to queſtion that theſe iſlands have been covered with water; and we have no leſs proof in *France*, that this part of *Europe* which we inhabit, has ſerved for a bed to the ſea. It is about 150 years ſince *Bernard Paliſſy*, a frenchman, without any other ſtudies than thoſe of his own obſervations made in the kingdom, began to inſinuate this doctrine in ſome publick conferences, which he held at *Paris* under *Henry* III.

I can render the thing more ſenſible and probable by adding to his obſervations thoſe, which ſeveral journeys undertaken in every part of the kindom to herboriſe, have given me an opportunity to make.

I had the honour ſome years ago to preſent to the academy ſome true *Madreporæ* ſtill adhering to their rocks, which I had looſened from the earth at *Chaumont* near *Geſorre*; they are ſtony plants found only at the bottom of the ſea, and are the moſt certain marks that can be had that this part of the continent was formerly a part of the baſon of the ſea.

I have ſeen alſo in the quarries of free ſtone of *St. Leu Taverni* ſome ſtones opened, in which the little ſhells and little *galets**, of which the baſon of moſt ſeas is uſually full, are incloſed; and I obſerved, that the ſurface of theſe beds of freeſtone is covered with a ſand, perfectly like that of the ſea ſhoar.

M. *Billeret* profeſſor of botany at *Bezançon*, has ſent me ſome pieces of rocks detatched from the quarries of the *Franche Comté* upon which were ſtill to be ſeen ſome of theſe tubes wrought by certain ſea-worms, which lodge in them, and ſuch

* A ſort of round or flat ſmooth pebbles found at the bottom of the ſea and rivers.

as

as are found in our ſeas upon the rocks,from which the coral is torn.

There are ſeen alſo in *Dauphiny* between *Gap* and *Siſteron* certain mountains, where the traces of the decreaſe of the waters of the ſea are marked by amphitheatres, the degrees of which increaſe in breadth in proportion as they approach the foot of the mountain.

As by this ancient poſition of the baſon of the ſea demonſtrated in ſeveral places of the heart of the kingdom, it is manifeſt that theſe places have been covered with water, it will be eaſily comprehended that impetuous waves driven from N. to S. and repelled from S. to N. either by the reſiſtance of the high mountains. or by violent hurricanes, have carried away with them the animals and plants of the ſouthern countries, whence theſe waves ebbed, and that in theſe refluxes the waters ſliding in, and remaining ſome time in places where ſome diſpoſitions of mountains formed a ſort of baſons, they retained theſe light bodies, ſome entire, and others broken.

Thus are theſe exotic plants brought by the ſea water very far upon our lands, it remains only to explain the manner in which the impreſſion of them was made, whilſt theſe waters were evaporated, and was preſerved after their retreat.

We ſuppoſe their leaves floating upon the ſurface of a water, which in its agitations was ſtill more loaded with a bituminous ſlime which it had diluted, than with the ſalt with which it was naturally impregnated. This ſlime has covered the ſurface of theſe floating leaves, has been retained there by the number of nerves with which they are traverſed, is united ſo intimately with them, as to take even the minuteſt traces, and has acquired ſo much the more conſiſtence, as theſe leaves by the

closeness of their texture, have longer resisted corruption. But however as they have rotted at last and as the slime which covered them could not help precipitating, either by the withdrawing of the body which sustained it, or that being become by this withdrawing more penetrable by the water it was more heavy; it is in this precipitation, that the slimy *laminæ* falling upon the smooth surfaces of a diluted mud, have marked thereon the figure of the leaves of which they had received the impression.

The explanation of this mechanism gives a plain solution of the singularity of the representation of one and the same face of these leaves of plants in *relievo* on one *lamina* and in hollow on the opposite. Just as a seal imprinted in *relievo* on a *lamina* of earth, appears hollow on another soft *lamina* on which the first has been applied.

We cannot say that one of these impressions, which are seen upon these *laminæ*, is that of the reverse of the leaf, whilst the other is that of the upper part, since the leaf being rotted is become incapable of imprinting this reverse. The rotting of it is so certain, that its substance having changed, has tinged these impressions black, and what has remained fastened to these *laminæ* has at most but made some impression less perfect, because this superfluity has filled the graving of the impression, and is now found in powder between some of these *laminæ* when theyare separated.

It will seem at first sight, that after the destruction of the leaf covered with slime, the water touching it immediately must have effaced the impressions which it had received; but if we attend to the quantity of bitumen, with which this slime abounds, and judge of it by the pit-coal which is found so frequently in this part of the *Lionnois*, and

that

that this bitumen is nothing but an oil of the earth, ſince it has taken more conſiſtence, cannot be diſſolved in water, we ſhall have no difficulty to comprehend, that the figures marked upon theſe ſlimy *laminæ* have been preſerved by being precipitated into the water; and that by making their impreſſions on other ſlimy ſurfaces, they are not incorporated with the ſlime upon which they fell, or with other *laminæ*, which were precipitated ſucceſſively upon them, ſince this bitumen mixed in theſe *laminæ*, had the ſame effect upon them as common oil mixed with paſte, to keep it in *laminæ*.

It muſt be preſumed, that a million of heaps of leaves of exotic plants have been thus tranſported by the ſame waters into ſeveral other parts of *Europe*; but that this bituminous matter being found only in ſome places, theſe places have been almoſt the only ones that have preſerved their impreſſion. Such are alſo the coal-pits in *Glouceſterſhire*, upon the ſtones of which are figured moſt of the ſame plants that are obſerved in thoſe of the pits of *Saint-Chaumont*.

As I think I have demonſtrated that theſe imprinted ſlimy and bituminous *laminæ* were precipitated ſucceſſively, and that the leaves, which ſupported theſe *laminæ*, were of unequal figure and bigneſs, we muſt not be ſurpriſed, that in their precipitation they fell different ways. This is the cauſe that in theſe *laminæ*, which form ſo many different leaves, there are found ſo many different magnitudes, applied to one another, and that ſome even ſeem to have been broken in their fall by their ſhock againſt other *laminæ*.

If the beds of theſe ſtones, which uſually are intermixed with beds of coal, ſeem in ſome places to have acquired an oblique ſituation, we can only

only afcribe this difpofition to the inequality of the bottom of the bafon in which the water was inclofed, wherein thefe precipitations are made.

Laftly, the *ftrata* of coal, which feparate the *ftrata* of thefe ftones, muft be confidered only as a bitumen, which having at firft been liquid, infinuated itfelf, and afterwards grew hard between thefe *ftrata* of figured ftones. The *petroleum*, which actually flows in *Auvergne*, and is there called *Pege*, as if one fhould fay liquid pitch, is a proof of it.

It is therefore unneceffary to have recourfe to the fports of nature, or to a fuppofed internal vegetation; that is, between two *laminæ* of ftone, nor to a *palingenefia*, as fome modern authors have done, to explain the manner of formation of the impreffions which we obferve upon thefe ftones, and upon the leaves of which they are compofed. And though we fhould pretend they are an effect of the confufion of the deluge, yet we could not by the obfervation of the impreffions which reprefent thefe plants in maturity and in feed, determine either the month or the feafon of this univerfal inundation, fince thefe plants being come from the hot countries, might come to feed earlier than they do here.

An explanation of the figures.

Plate VII. *Fig.* 8. A fragment of an exotic fern imprinted on a black flate ftone about *Saint-Chaumont.*

Fig. 9. A fragment of a leaf of another exotic plant imprinted on a ftone almoft of the fame fort.

Fig. 10. Fragments of feveral leaves of another fort of exotic fern, the figures of which are applied

different ways with regard to one another, upon a talky ſtone of the ſame place.

Fig. 11. Parcels of leaves or *pinnulæ* of a third ſort of exotic fern, the impreſſions of which are ſcattered in different places of the ſurface of one of theſe ſorts of ſtones.

Fig. 12. A fragment of an exotic *Filicula* imprinted in hollow upon a hard greyiſh ſtone of the ſame place.

Fig. 13. A. a fragment of another *filicula* imprinted in hollow upon a tender and talky ſtone of the ſame place.

B. A *lamina* or leaf raiſed from the preceding ſtone, repreſenting in *relievo* the ſame ſide of the *filicula* imprinted in hollow upon the *lamina* A.

Fig. 14. A fragment of a ſtalk of an exotic capillary plant found in the ſame place.

Fig. 15. A fragment of ſtone from the ſame place, in the middle of which are the traces of a round leaf of an exotic plant, or of a ſeed approaching to that of an elm.

VIII. *Obſervations on a northern light; by M.* Maraldi *.

We have obſerved the northern light twice this autumn. It appeared the firſt time, *September* 16, a little after eight at night. The ſky being very clear, we ſaw bright exhalations, which coming out of a ſort of a fog at the horizon, roſe 12 or 15°, and formed a very vivid light. This light at firſt occupied a ſmall portion of the horizon toward the north, and gradually diminiſhed; ſo that at $8^{h}\frac{3}{4}$ it was hardly viſible, but afterwards it increaſed conſiderably all at once, both in magnitude and brightneſs,

* Dec. 14, 1728.

being

being extended toward the N. and N. E. and it appeared very bright and luminous for $\frac{3}{4}$ of an hour, that is from $8^h \frac{3}{4}$ to $9^h \frac{1}{2}$. In this ſtate it roſe a little above the ſtars of the preceding paw, and of the poſterior knee of the *Great Bear*. Its bound on the N. W. was above the tail of the *Great Bear*, and on the N. E. ſide it was more eaſterly than the preceding paw, After $9 \frac{1}{2}$ it diminiſhed a few minutes, having begun to fade at its eaſtern part, there remained a faint track of it quite to the riſing of the moon.

We obſerved it the ſecond time, *Nov.* 23, at $10^h \frac{3}{4}$ at night. The ſky being quite covered, we ſaw the light come only out of one place of the horizon, very near the north, and it illuminated the clouds which were on that ſide, riſing by intervals, and ſpreading upwards to 10 or 12° in height. It was not continued from the bottom to the top, but we only ſaw the lower part near the horizon, and the upper part that was moſt diſtant; the middle, which was two degrees in breath, being hid by a cloud parallel to the horizon. Thus there were ſome clouds nearer us, and others farther off, than the exhalations which formed the light; after having obſerved it for half an hour, it diſappeared.

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Fig. 1.
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Fig. 12.
Fig. 13.
Fig. 13.
Fig. 14.
Fig. 15.
J. Mynde sc.

A TABLE

OF THE

PAPERS contained in the ABRIDGMENT of the HISTORY and MEMOIRS of the ROYAL ACADEMY of SCIENCES at PARIS, for the Year MDCCXIX.

In the HISTORY.

In the MEMOIRS.

I. *Meteorological obſervations made at the royal obſervatory, during the year* 1718, *by M.* de la Hire, *the elder brother.*

II. *Reflections upon ſeveral obſervations concerning the nature of the* gypſum; *by M.* de Juſſieu.

III. *On the method of procuring quickſilver from the mines of* Almaden *in* Spain, *and the nature of the diſeaſes of thoſe employed therein*; *by M.* de Juſſieu.

IV. *A compariſon of ſome obſervations made by the* chevalier de Louville, *with thoſe which have been made at the obſervatory*; *by M.* Maraldi.

An

AN

ABRIDGMENT

OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the year 1719.

I. *On the* aurora borealis, *or northern light; translated by Mr.* Chambers.

THE light above treated of, appeared again in the preſent year at different times thereof, and in different countries; it had ſcarce been ſeen at *Paris* but by the profeſſed obſervers, till on the 30th of *March* at $\frac{1}{4}$ paſt 8 in the evening, when it became a popular *phœnomenon*; the people in the ſtreets were all aſtoniſhed at it, and a cry of admiration roſe at once through the whole city. M. *Maraldi* fortunately happened to be a ſpectator of the meteor; it appeared as a column of fire about 20 degrees above the horizon, and extended almoſt parallelly thereto over a ſpace of 25 or 30 degrees, being pointed at its weſtern extream, and ſomewhat above the ſemi-diameter of the ſun at its eaſtern one, the upper part all along appeared brighter than the lower, which was very red; but the whole effaced the light of the moon, though then in her 8th day, and very clear, the ſky being perfectly ſerene. The ſituation of the meteor was between N. N. W. and W. and it ſeemed to have a little motion towards the W. but M. *Maraldi* had hardly time to ob-

ſerve it for a few ſeconds, ere it diſappeared all at once, without altering its poſition with regard to the horizon.

On the 7th of *April*, at 9 in the evening, M. *Maraldi* obſerved another meteor equally vivid with the former, but not ſo ſtill nor uniform as it, nor of ſo ſhort duration; it appeared from the north eaſt to the north weſt, and by the columns which roſe from it from time to time, had diſappeared again, reſembling that already treated of for the year 1716; it laſted near an hour and ½.

When ſuch meteors are not ſtill, but happen to be agitated, their agitation is uſually found to be much the ſame; there is in all of them a fund or baſe of light, from whence vertical columns ſucceſſively ariſe. 'Tis probable that in the general kindling there are ſome matters which have not yet taken fire, but which catch it one after another, beginning always to burn at the bottom, as being there moſt inflammable.

The like meteors were ſeen on the 22d of *Feb.* at *Vicenza* and *Bologna*, and on the 25th of *March*; and at *Montauban* in *Languedoc* an hour or two after ſun-ſet: that ſeen in *Italy* could not by *Manfredi*'s calculation be leſs than four leagues above the ſurface of the earth.

It appears therefore upon the whole, that for four years, commencing from the year 1716 incluſive, this meteor has appeared very frequently, though during all the ages paſt we have no accounts of above four appearances.

The generality of them were of the ſtill kind, and appeared one or two hours after ſun-ſet, in a moderately cold, but very dry ſeaſon, and they appeared in different parts of *Europe*, at very ſmall intervals of time. This laſt circumſtance ſhews that the ſame diſpoſition obtained at the

ſame

ſame time through a large extent of the atmoſphere, which M. *Maraldi* had before obſerved by the variations of the atmoſphere.

II. *On the general cauſe of cold in winter, and heat in ſummer.*

That which is no doubt among philoſophers, is ſometimes a doubt among the generality; and on the contrary, what is no doubt among the generality, is often a doubt among philoſophers. There are but few elſe who can find difficulties in the general cauſe of cold in winter, and heat in ſummer, and in the particulars of this whole affair. M. *de Mairan* has been of opinion, that hitherto it has been but ſlightly examined, tho' it well deſerves it.

It ſtrikes one immediately, that this general cauſe is the different elevation of the ſun above the horizon in ſummer and winter, but yet it requires to be diſcuſſed, and this will occaſion ſome difficulty.

The rays of the ſun, as well as all other bodies, ſtrike a plane, which receives them with ſo much the more force as they are leſs inclined and on the contrary; and it is plain that in ſummer they are leſs inclined to the ſurface of the earth, if we take this ſurface to be exactly ſpherical, and it muſt needs be ſo eſpecially with regard to the rays of the ſun. The ſmootheſt plane (and what muſt the reſt be?) is an aſſemblage of an infinite number of ſmall planes differently inclined, and receiving rays from the ſun under all the angles poſſible, ſo that the general inclination of the climate is not to be reckoned.

For this reaſon ſome believe that it is not upon the unequal ſurface of the earth that this inclination

of

of the climate muſt be taken, but upon that of the atmoſphere, which is perfectly ſmooth, becauſe it is a fluid, and a ſtill fluid, at leaſt in its upper region. That part of the atmoſphere correſponding to the climate of *Paris*, for example, will therefore be more vigorouſly ſtruck in ſummer by the rays of the ſun, and conſequently more heated, and will heat *Paris*. This may be: but beſides this *mediate* heat communicated to the earth by the atmoſphere, which does not hinder its being always very cold upon the high mountains; there is manifeſtly another cauſed immediately upon the earth by the leſs inclined rays of the ſun, which we now principally conſider.

Some philoſophers refer alſo to the atmoſphere, but in a manner ſomething different, the cauſe of the heat and cold. They ſay that bodies being as much more eaſily reflected by a ſurface upon which they fall, as they fall more obliquely upon it, (as appears by the example of ducks and drakes that are made upon the water with ſtones, which would penetrate it, if they fell with leſs inclination,) the atmoſphere reflects by its upper and convex ſurface ſo many more rays, and conſequently lets as many leſs of them, paſs to the earth, as they fall more inclined, and on the contrary.

But M. *Mairan* pretends that by all the experiments, which have been made, and by thoſe which he himſelf has made upon light, it does not appear that when its incidence is more oblique, the quantity is greater that is reflected from it, in proportion to what is refracted. The example of the ducks and drakes would not draw a conſequence for light, which has its properties apart. It is true, and it is a neceſſary conſequence of the conſtant and determined proportion of the ſines of incidence and refraction, that when light paſſes from a denſer

ſer medium, into one more rare, as from water into air, there is a certain determined obliquity, after which every ray which has a more oblique incidence cannot but be reflected, and is not refracted to paſs into the ſecond *medium*, which perfectly repreſents the duck and drake. But here we are in the oppoſite caſe, where the light paſſes from a more rare *medium*, into a more denſe one, from the æther into the atmoſphere, and that caſe equally permits all the rays to be refracted, whatſoever their incidence may be; and we have no room to ſuſpect that in the moſt oblique incicidences there are more reflected than refracted.

A great aſtronomer has thought that the heat increaſed by an incidence of rays upon the earth, approaching more to a perpendicularity, becauſe a perpendicular ray reflecting upon it ſelf heats a ſecond time the air, which it has already heated; and that the other rays do it in proportion as they approach more to be perpendicular. But it is viſible that it does not any way ſignify that it ſhould be the ſame air that is heated a ſecond time by the reflection of the ſame rays, and that all reflected rays will heat a ſecond time an air already heated by ſome other incident ray, perfectly equal in force to the firſt.

We cannot draw any thing from the greateſt or leaſt diſtance of the ſun from the earth, when it is in its *apogeum* or in its *perigeum*. It is now in its *perigeum* at the end of *December*, and this greateſt proximity does not at all ſoften the rigour of our winters, and will but little increaſe the heat of the ſummers in our climate, when in a great number of ages the *perigeum* ſhall be in the month of *June*, in effect, this great proximity is only $\frac{1}{30}$ part of the total diſtance of the ſun from the earth.

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M. *Mairan* has therefore recourſe to other principles, he looks upon light as a fluid, and this is certainly the moſt natural idea that we can frame of it. A ſurface being determined, a fluid which we muſt conceive as divided into an infinite number of threads parallel between themſelves, ſtrikes it with all its threads, if it ſtrikes it perpendicularly; and does not ſtrike with any, if it is parallel to it; from whence it follows that the more obliquely it ſtrikes it, the ſmaller is the quantity of thread by which it is ſtruck, and on the contrary. It is the ſame with the force of the *impetus* of each thread as of their number, the *impetus* has all its force poſſible when the thread is perpendicular to the ſurface, and when it is parallel this force is null, ſince there is no *impetus* conſequently both the number and force of the threads depending upon their angle of incidence upon the ſurface, they are each of them meaſured by the ſine of this angle, and both together by the ſquare of this ſine.

We find by the tables, that at *Paris*, the ſine of the meridian height of the ſun upon the horizon at the ſummer ſolſtice, is to the ſine of its meridian height at the winter ſolſtice, pretty near as 3 to 1, and conſequently the whole effect of the rays of the ſun, or the noon day heat, muſt be nine times greater than that of the other.

This reaſoning is not true in its whole extent, and without modification, but with regard to the portion of the exteriour ſurface of the atmoſphere, correſponding to our climate. It ſubſiſts alſo with regard to the earth or *Paris* as to the quantity of rays, but not as to the force of the *impetus*, for, as we have ſeen, their incidence is always very different upon planes differently inclined to the ground of *Paris*, M. *de Mairan* ſupplies this by a reflection, he imagines and has alſo obſerved that all ſmall planes

planes differently inclined, which forms the unequal ſurface of a ground, being ſtruck by the rays of the ſun, throw ſhadows on the oppoſite ſide, that theſe ſhadows being ſo much the longer as the ſun is leſs elevated, they cool, as one may ſay, a greater number of neighbouring planes, and that therefore the earth is ſo much the leſs heated, and that it is the contrary when the ſun is more elevated; then the mixture of ſhadow being much leſs, almoſt all is on fire. By this means the effect of the force of the rays of the ſun follows, as well as their quantity, the different heights of the ſun upon the horizon.

To the two principles already found of a greater heat at the ſummer ſolſtice, he adds a third which muſt have a great deal of effect. The more the rays are inclined, the greater thickneſs of the atmoſphere they have to croſs, or the greater way they make, and conſequently they meet ſo many more ſolid parts which intercept, or extinguiſh them, and on the contrary. But we cannot be aſſured that this third principle follows, as the two other do, the proportion of the ſines of the height of the ſun. The reaſon of this difference is, that the firſt and ſecond are contained between two fixed and determined points, one of which is the perpendicularity where the rays have their whole force, and their intire number, and the other the paralleliſm where they have no force, and are not in any number; but the third principle is not of this nature; in the caſe of perpendicularity itſelf, there are alſo rays intercepted or extinguiſhed by the atmoſphere; and we cannot tell what quantity there is of them, nor how much greater they are for each angle of inclination. It is therefore only by eſtimation that we

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can meaſure this third principle, and therefore the eſtimate muſt be different for each climate.

The augmentation or diminution of light follows that of heat, and it is certain that a country is ſenſibly leſs illuminated in winter than in ſummer. M. *Mairan* has obſerved in the eclipſes of the ſun, that when half its diſk is covered, and that conſequently it ſends us but half its rays, yet there is not any diminution of light that is ſenſible; and from hence he judges pretty certainly that when it is ſo, as in winter, there is then a dimunition of more than half its rays. He takes but the half to avoid ſwelling his calculation; and conſequently there has been twice as many rays which penetrate the atmoſphere, and come to us; or, which is the ſame thing, the third principle multiplied by two, the product of the two others, which was nine for *Paris*; or, laſtly, the heat of the ſummer ſolſtice is eighteen times greater than that of the winter ſolſtice.

This would be geometrically true, if the ſun was one moment at the ſummer ſolſtice, and in the next at the winter ſolſtice; and if we compared theſe two moments together, but phyſically this is not, nor cannot be ſo. The ſun gives greater heat to the grounds already heated; and hence it is that after the ſummer ſolſtice the heat is greater than before this ſolſtice, at the ſame height of the ſun. There is in the action of heat a ſort of acceleration, but we cannot keep an exact account of it, and ſo much the leſs, as it is interrupted by the nights, and always unequally interrupted in our oblique ſphere. But, laſtly, there is always ſome remainder of acceleration, which augments the proportion of 18 to 1 of any value. It muſt go a good way, if we judge of it by the very ſenſible difference of the heat which follows

follows the ſummer ſolſtice to that which preceeds it. It is the ſame thing with the winter ſolſtice, which is followed by a much greater cold than that which preceeds it. This acceleration of heat or cold is commonly in its greateſt force forty days after both the ſolſtices.

Paſſing over all the particular and probably inſurmountable difficulties which are found in the calculation of the acceleration of heat, M. *Mairan* judges in general that it muſt be proportioned to the length of the days which are toward the ſummer ſolſtice. Now they are then at *Paris* twice as long as thoſe of the winter ſolſtice. But there is beſides, not only that the ſun has been twice as long above the horizon, but it has had three times as much force. It is true that this force ſeems to have been already employed, when we have found the proportion of 9 to 1 for the ſummer ſolſtice, and that of the winter; but it muſt be obſerved that this is not the ſame force. The firſt was that of the rays of one ſolſtice compared to thoſe of another; the ſecond, of which we hear ſpeak, is that which remains from the day of the ſummer ſolſtice to the following, and from this to another, ſince it is only in this that the acceleration conſiſts. We may therefore take the proportion of 6 to 1 for that of the acceleration of heat cauſed by the ſummer ſolſtice, or increaſing from this ſolſtice, to the acceleration of heat decreaſing to the winter ſolſtice, or which is the ſame thing, to the cold of this ſolſtice. But M. *de Mairan*, to ſet all at the loweſt rate, takes only the proportion of 4 to 1, which multiplying the proportion of 18 to 1, gives him for *Paris* the greateſt heat of ſummer ſeventy-two times greater than that of winter.

He has however been ſo ſcrupulous as not to keep yet to this proportion. He has conſidered that the proportion of the ſines of the winter ſolſtice is not exactly that of 3 to 1; that the refraction elevating the ſun, though unequally, makes its rays remain a longer time above the horizon; and laſtly, that it is nearer the earth during our winter: and all this being reckoned, reduces the proportion ſought to be that of 66 to 1.

M. *de Mairan* himſelf makes an objection, which ſeems at firſt to overthrow his theory and calculation irrecoverably. We have ſaid in the hiſtory of 1702, that by M. *Amontons*'s obſervations, *the heat that is made at* Paris *by the rays of the ſun at noon in the ſummer ſolſtice, differs from the cold that is there when water freezes, only as* 60 *differs from* 51 $\frac{1}{2}$, *or nearly as* 8 *from* 7. *What becomes then of the proportion of* 66 *to* 1?

M. *Amontons*'s experiments were made with the thermometer, which feels, if I may ſo ſay, all the heat that is in a place, and gives an account of it. Thus at the winter ſolſtice there are at *Paris* $51^{\circ} \frac{1}{2}$ of heat, and 60° at the ſummer ſolſtice. But M. *de Mairan*'s calculation marks only the proportion of the heat that the ſun produces in winter, to that which is in ſummer; ſo that if there was a fund of heat independent of the ſun, cauſed either by the continual agitation of the ſubtile matter, or by the ſubterraneous fires; or elſe if the earth ſhall at length acquire by the action of the ſun, and not loſe it any more, the heat of the winter will be one degree, which will be added to this fund of heat, and the heat of ſummer 66 degrees. Now it is eaſy to find a number, that by adding to it 1 on one ſide, and 66 on the other, the two new numbers reſulting from theſe additions, may be as 51 $\frac{1}{2}$ to 60. This number is 393 within a fraction;

fraction; so that we have 393 for the constant and perpetual fund of heat of the climate of *Paris*, to which the action of the sun adds 1 to the winter solstice, and 66 to the summer solstice.

III. *On the wasps.*

The wasps are no less the object of the contempt and aversion of men, than bees are of their favour and esteem. Their work is absolutely useless to us, and they are enemies to the bees which are our favourites. But as it is not what is useful or agreeable to us that ought to regulate the orders of beings in the universe, wasps and bees are pretty equal in the eyes of philosophers; which in this, if it is not too presumptuous a thought, imitate in some measure the eyes of the creator. After the study that M. *Maraldi* has made of the bees, which we have given an account of in 1712, M. *de Reaumur* has done as much upon the wasps, another nation less known, less polite, and more gross; and which are with regard to the bees, what the *Spartans* were with regard to the *Athenians*. We are going to see the difference and the conformity of the two governments, supposing that of the bees to be known by the history of 1712.

All wasps, as well as bees, construct an edifice, which consists of combs with hexagonal cells. M. *de Reaumur* distinguishes three sorts of wasps with regard to their different places where they inhabit. The first build them in the open air upon branches of trees, the second in trunks or in granaries that are but little frequented, and the last under ground. The second sort are hornets; they are the biggest of all; the third are the most common in this country; and though the situation

of

of their neſt renders them more difficult to be obſerved, at leaſt as to the inſide of their ſtate; it is however upon theſe that M. *Reaumur* has made his principal obſervations. He has had the art, and even courage, for this is not done without ſome danger, to diſcover the neſts in their holes, and remove them into glaſs hives, where they were expoſed to view as the works of bees are.

A neſt is commonly round, and about 13 or 14 inches in diameter; the materials that it is made with, is more like fine paper than any thing elſe. We ſee at firſt a pretty thick covering, which is of many leaves of this paper put one upon another. It muſt not however be imagined that each of theſe leaves are one piece, or that they make one ſingle plane; each is in ſeveral pieces in the ſhape of an oyſter-ſhell, and have their convexity on the outſide; theſe ſort of ſhells are, as it were, glued together in a very ſenſible manner, at the edges, and there they are pretty eaſily ſeparated. Several leaves or layers thus formed neceſſarily leave great ſpaces between; ſo that in the whole covering, which they compoſe, although pretty thick, there enters but little matter. The ſpace contained in the inner ſurface of this covering, is cut by 15 plains at moſt, placed horizontally upon one another, and conſequently, becauſe of the ſpherical figure of the neſt; ſo that the firſt and laſt are the leaſt, and the middle one the greateſt. There are intervals of ½ an inch left between them; they are faſtened by their edges to the inner ſurface of the covering, and are hung to one another by ligaments diſpoſed from ſpace to ſpace, they are exactly of the ſame matter with the common covering, and all have hexagonal cells in their lower ſurface, of the ſame matter. Theſe are the combs or cakes

of

of the waſps. The cells are only deſigned to receive their eggs, and there is no proviſion in them as in thoſe of the bees. The common covering of the neſt is pierced by two holes at a diſtance from one another, one of which is only uſed for the entrance of the waſps, and the other only for their going out. It is ſeldom that any bold or ventureſome waſp changes the uſe of either of theſe doors.

As almoſt all the waſps periſh in the winter, we may ſuppoſe that at the beginning of the ſpring there remains only 10 or 12 in a neſt, or even only one; for it is poſſible that one may be ſufficient to reſtore the nation. This waſp will forſake the old neſt where it has lived till then, and will undertake to conſtruct another, or rather lay the foundation of it. For this purpoſe it goes into the fields, and gathers together wherewithal to make its paper, which is little bits of woods, as ſaw-duſt taken from certain woods, ſuch as the props of vines, window-ſhutters, &c. the ſurface of which having been often wet with the rain, is become ſofter by it; in the gnawing it endeavours to detach threads from it. Theſe threads glue together either by their natural viſcoſity, or by the addition of ſome glutinous liquor from the waſps; they compoſe then a ſort of paſte which ſpreads and draws eaſily, like that of which the paper is made. Of theſe materials the waſp builds the firſt little dome of the ſpherical neſt, which it has glued on the outſide to ſome ſolid place, and it does not forget to faſten ligaments on the inſide, which are to hold the firſt cake ſuſpended horizontally. It proceeds in the ſame manner to the conſtruction of the ſecond cake, and at the ſame time makes the portion of the common covering which is to anſwer to it. In each

each cake, and with the ſame matter, it raiſes the walls of the hexagonal cells, but yet in ſuch a manner, that thoſe which are exactly at the edge of the cake are not hexagonal, but only have three faces toward the centre of the cake, where they are touched by other cells, they are ſemicircular on the other ſide, which is the edge of the cake. As the cells at the centre are firſt built, each has been in the time of its conſtruction an outward cell, and conſequently was a cell with three faces on one ſide, and ſemicircular on the other.

As ſoon as there are a few cells made, the waſp lays an egg in each of them. It was fecundated in the preceding *October*, and during the whole winter which it has paſſed without eating, the principle of this fecundation remained alſo without action, and waited for the ſpring to put the eggs in a ſtate and neceſſity of coming out. The egg hatched is at firſt a worm, and afterwards a fly, in the ſame manner as the bees, and a thouſand other inſects are. We ſhall leave the hiſtory of the egg and its metamorphoſis to M. *de Reaumur*. The waſp muſt have nouriſhed the little worms its infants; it brings them either juices which it has taken upon plants, and which it diſcharges in its neſt, or bellies of inſects, or even bits of meat ſtolen from the butcher's ſhops, and ſometimes almoſt as big as itſelf.

As in general the worms that metamorphoſe themſelves and take wings, have all their magnitude and ſtrength as ſoon as they are winged, the new-born waſps aſſiſt their mother in continuing the conſtruction of the neſt which has not at all advanced; and in proportion as it advances, which is very faſt, becauſe there are a great many labourers, the mother lays new eggs in the new cells,

cells, and the young waſps help it alſo to nouriſh their brothers and ſiſters. According to this conſtant order, the neſt is finiſhed and filled with inhabitants.

This ſuppoſes a fruitfulneſs in one ſingle waſp, for all is proceeded from it. There are at leaſt ten thouſand cells in a neſt, each of which has received an egg. But beſides each receives ſucceſſively two or three of them in a ſummer; and at this rate one fly would produce 25 or 30 thouſand of them. But we have already ſeen that the king of the bees may be as fruitful: it is neceſſary that in a ſpecies which periſh almoſt entirely in the winter, and is very numerous in the ſummer, the individuals which repair it ſhould be prodigiouſly fruitful; ſeveral fiſhes give us the idea of a yet greater fruitfulneſs. In ſhort, it does not belong to our imaginations and our ſhort experience to preſcribe bounds to any thing.

There are in a hive of bees, as we have ſeen in 1712, three ſorts of flies. The bees, properly ſo called, which are all barren, neither males nor females; the king, which of himſelf makes a ſpecies alone, and is the only one which lays eggs, is conſequently female, and would better deſerve the name of queen; the drones, which are all males, and the males of the queen alone, ſhe reſembles ſome queens of the *Eaſt* and *Africa*, which are ſaid to have ſeraglio's of men. M. *de Reaumur* has diſtinguiſhed alſo among the waſps theſe three ſorts: he calls thoſe mules which are neither males nor females, and conſequently barren; they deſerve this name alſo by being the ſtrongeſt and moſt laborious of all. There is not only one female, or at moſt two or three, as among the bees, but ſome hundreds. It is the ſame with the males, and the number of the

mules is without comparifon the greateft. They are alfo the fmalleft in the fpecies, the males are the next, and then the females. Thefe laft have a great heavy belly, which agrees with the prodigious quantity of eggs with which they are charged. The males which are of the figure of the drones among the bees, are alfo like them in this, that they have no fting; the mules and females are provided with them.

The offices are well enough diftributed in this republick, and almoft as in that of the bees. The mules, which becaufe of their barrennefs are ufelefs for futurity, carry at prefent the heavieft burdens; they go to feek for the materials for the building, they conftruct it, they go to the chafe, and for provifions as well for themfelves as for all the other flies remaining in the neft, and even for the little ones. The females never build unlefs it be at the beginning of the fpring, when the number of labourers being then very fmall, they are all forced to fet their hand to the work. As to the reft, the care of their young ones alone imploys them. The males have a leifure joined with dignity, and this dignity is being males, the propagators of the fpecies, and the guardians of its perpetuity.

One thing which is fingular, and which M. *de Reaumur* has difcovered, is that when the fingle wafp, as we have fuppofed at the beginning, or feveral which have furvived the reft of the nation extinguifhed by the rigour of the winter, fet themfelves, at the return of the fpring, to re-eftablifh it by laying their eggs, they lay at firft, and for fome time only the eggs, which muft hatch mules; and during this time, both they and the young wafps, companions of their work, only build cells to lodge the eggs of mules; for the

mules

mules being much the ſmalleſt, and likewiſe the eggs from whence they proceed, they only require the ſmalleſt cells. They afterwards build greater cells, and fill them with the largeſt eggs, which are thoſe of the males and females. Although the males and females differ in ſize, the eggs that produce them do not differ enough to require unequal cells. It appears that in the regeneration of the ſpecies, the mules are produced the firſt, becauſe they are the greateſt workers, and the moſt neceſſary for the growing ſtate. Nature muſt therefore have ranged the eggs in the ovary of a female waſp according to a certain order, or has diſpoſed their fecundation ſo as not to proceed but according to this order.

The copulation of the males and females is viſible, and M. *de Reaumur* has given an exact account of it. It is in *October*, as is that of all other flies. As there are in one neſt two or three hundred male waſps, and as many females, it would be difficult for ſo great a people to conceal their amours like the queen of the bees, who is ſingle, or has but few companions; not to mention that the waſps perhaps may not have naturally ſuch abundance of modeſty. Which way ſoever it is, the waſps have betrayed the myſterious queen of the bees; for M. *de Reaumur*, after having ſeen the copulation of the male and female waſps, does not queſtion there being the ſame between the drones and this queen.

We have ſeen in the hiſtory of the bees, that at the beginning of the cold, or of the winter, they declare war againſt the drones, kill them or drive them away, and even deſtroy all the eggs which would become drones. The waſps are yet worſe at that ſeaſon; they deſtroy all the eggs and all the young ones without exception. The

mules and males work at it with fury; there are only females or the mothers, that M. *de Reaumur* has not sufficiently convicted of barbarity. Perhaps after all, this is only an appearance of barbarity, the wasps make no provisions for the winter, and they save their little ones a great deal of pining and suffering.

They reserve that for themselves; they eat nothing during the winter, even though they are sollicited to it by presenting to them what they are fondest of. All the mules and males die, there only escape some females, perhaps a single one in the whole nest. They were fecundated in the preceding *October*, and that is the recovery of the nation, which at the beginning of the spring will be repaired, as we have said.

What diversity reigns among the different species of animals with regard to generation? There are some where all are males and females at the same time, others where there is hardly male or female, and where all are without sex except a few individuals. Nature seems to have taken pleasure in following the rules of combination, and the more we compare together its different works, the more we shall find this genius of combination to prevail. Perhaps by following this idea we might sometimes make a pretty happy conjecture.

Explanation of the figures in the history of the wasps.

Plate VIII. represents the principal species of these insects, and some of their parts drawn separately, seen through a microscope.

Fig. 1. is a wasp of the most common species in this country; it is one of the third class, or subterraneous

terraneous waſps, and is of the gender with thoſe which we have called mules.

Fig. 2 and 3. are waſps of the ſame claſs and ſpecies with the preceding, but they are the males of this ſpecies.

Fig. 4 and 5. are the females of the ſame ſpecies.

Fig. 6 is, as *fig.* 1. that of a mule, but ſmaller; it is found in the ſame neſt. There are much fewer of theſe little mules than of the others.

Fig. 7. is a mother waſp of *fig.* 4 and 5, ſeen on the ſide of the belly.

Fig. 8. is the head of one of the preceding waſps, enlarged by a microſcope, and ſeen in front. The heads of the mothers, males and mules, do not differ ſenſibly from one another but in bigneſs; a a the *antennæ*; b b the eyes like thoſe of other flies; c c the ſaws or moveable teeth, which ſerve them for many uſes.

Fig. 9. is the head of a waſp of the ſame ſpecies ſeen underneath, the ſaws are removed, and the trunk preſſed towards its origin to oblige it to lengthen; c c the two ſaws; d d the trunk.

Fig. 10. the ſame head ſeen above; c c the ſaws; d the trunk; b b the *antennæ*.

Fig. 11. part of the trunk repreſented ſeparately.

Fig. 12. the ſaws repreſently ſeparately.

Fig. 13. a worm which is to become a mother waſp; a is the head of it.

Fig. 14 and 15. are thoſe of the worm of *fig.* 13. when it is transformed into a *chryſalis*.

Fig. 14. repreſents the *chryſalis* ſeen on the ſide of the back; and

Fig. 15. repreſents it ſeen on the ſide of the belly.

Fig. 16. is the *chryſalis* ready to become a waſp; m is a portion of the caſe which covers all

all the parts of the infect which has been drawn downwards. This cafe is made of a thin white membrane, fo that it is not feen upon the parts which it covers, when it is fpread upon it; but if we rub thefe parts, we loofen this membrane, and then it becomes fenfible, and the parts from whence we take it away, appear of a more lively colour, and not fo white as before.

Fig. 17. is the fcaly part which terminates the body of the male wafps of *fig.* 2 and 3. This part is reprefented magnified by the microfcope; e is the brown and fcaly part which holds to the laft of the rings; ff the two fcaly pinchers between which is the part of the infect intended for generation; g the end of the part made like a fpoon, and feen on the hollow fide of the fpoon.

Fig. 18. are the fame parts as thofe of *fig.* 17. but more magnified by the microfcope, and feen on the oppofite fide; h the two pinchers; K the fpoon, of which we fee the bowl in the convex fide; i i are perhaps fpermatick veffels.

Fig. 19. the part intended for generation reprefented feparately; I is the body or handle of the fpoon, of which g is the bowl.

Fig. 20. is a mother wafp of the fecond clafs, or hornet.

Fig. 21. the male of the hornets; the mules are not very different from it.

Fig. 22. the head of the hornets magnified and feen underneath.

Fig. 23 and 24. are the parts of the male hornets feen at the top and underneath; f f the two brown and fcaly pinchers; K g the parts which diftinguifh the male.

Fig. 25 and 26. wafps of the firft clafs. We have not thought it neceffary to reprefent here the differences which are between the males, the females,

males, and the mules; the differences in thickneſs and magnitude are ſenſible, but they are not very conſiderable.

Fig. 27. is a worm of this claſs ready to change into a *chryſalis*; a is the head of it.

Fig. 28. is the head of a worm magnified and repreſented ſeen on the face; b is the opening of the mouth; I call it mouth, becauſe there does not appear any tube which forms a trunk, as there is one in the flies, to which ſtate this worm is to arrive.

Fig. 29. repreſents a neſt ſeen on the outſide. It is however difficult to be able to form a clear idea upon this figure, in which all the parts are conſiderably leſs than nature. This neſt is almoſt 14 inches in its greateſt diameter.

Fig. 30. is a little bit of this neſt repreſented pretty near of the natural ſize; a is a waſp which labours at inlarging and cloſing an arch.

Fig. 31. is a portion of the covering of the neſt cut vertically, to ſhew how the arches are placed one upon another, and the vacuities which they leave between.

Fig. 32. repreſents only one of thoſe arched pieces, the aſſemblage of which forms the covering of the neſt.

Fig. 33. is a neſt, of which part of the covering is taken away to diſcover the inſide; a a is the covering that remains; bb, cc, dd, ee, ff, *&c.* mark different cakes which poſſeſs the inſide of the neſt. The apertures of the cells appear underneath the cakes: the apertures of the cells of the upper cakes bb, cc, dd, ee, are leſs than thoſe of the cells of the lower cakes ff, gg; the mules are raiſed in the cells of the firſt, the males and females in thoſe of the laſt. Theſe laſt cakes gg, hh, are not ſo thick as the others, but it is becauſe they

they are not quite finiſhed. An egg or a worm is often depoſited in a cell which is but juſt begun. Between two cakes we ſee the lines which ſuſpend the lower ones to the upper ones.

Fig. 34. is one of the cakes repreſented ſeparately, and ſeen above; PPP mark ſome of the lines which ſuſpend this cake.

Fig. 35. a cake ſeen reverſed, that the apertures of the cells may appear upwards. It is eaſy to diſtinguiſh therein the open cells from thoſe which have been ſtopped by the worms ready to be metamorphoſed. Some flies are ready to come out of ſome of the cells. Some flies are entered into others with their head foremoſt, and ſhew only the end of their tail.

Fig. 36. Some cells repreſented almoſt of their natural ſize, that their covering may be more ſenſible, and that the ſtructure of the cells may be the better ſeen.

Plate IX. *Fig.* 1, 2, and 3, repreſent fragments of hornets neſts. *Fig.* 1, ſhews portions of arches placed one upon another. *Fig.* 2 and 3, ſhew pieces of theſe arches, the inſide of their neſt being diſpoſed like thoſe of the ſubterraneous waſps, we have thought it unneceſſary to draw them beſides there are two figures of them in *Aldrovandus.*

Fig. 4. are cells of theſe waſps repreſented ſeparately, and almoſt of their natural ſize; ſome of them have their covers, which form hollow hemiſpheres.

Fig. 5. is a waſp's neſt of the firſt claſs; it conſiſts of one ſingle cake fixed againſt a branch of a tree.

Fig. 6. is the ſame cake ſeen on the back-part. The back part of this has alſo cells, ſome of which are marked by c c c c, but theſe cells are not common


Pl. VIII. Vol. V. p. 376.
Fig. 1.
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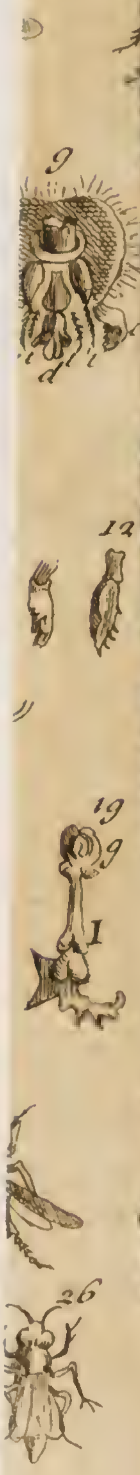
Pl. VIII. *Vol. V. p. 376.*



J. Mynde sc.

mon to theſe ſort of cakes; they are united on that ſide like thoſe in *fig.* 34. plate VIII.

Fig. 7. repreſents a neſt of the firſt claſs of waſps, which was brought to the academy by M. *Varignon*, it reſembles a double roſe not yet blown. They did not obſerve in what ſituation this neſt was in the tree; but to judge of it by that of the others, its poſition is reverſed in the drawing, o is the aperture of the neſt which was underneath, or placed horizontally; a a the covering of the neſt, compoſed of a great number of leaves applied upon one another, like the leaves of roſes, but much larger; b b little branches of the tree to which the neſt was faſtened.

Fig. 8. is the ſame neſt, of which I have cut part of its covering to ſhew the inſide, o is the aperture by which the flies enter, d e, d e, d e, mark the ſection of the covering; we ſee the different leaves placed one upon another, of which it is compoſed; g G the two cakes which fill the inſide of the neſt, the worms have almoſt all ſtopped their cells, and are ready to be metamorphoſed, as it is eaſy to judge by the raiſed cover which appears above the cells.

Fig. 9. is the cake g repreſented ſeparately, and is ſeen on the ſide oppoſite to the aperture of the cells, H is the line or foot which fixes this cake to the cake G.

Aldrovandus has given us the figure of a neſt made in the ſhape of a bottle, which is, I believe, the work of the ſame ſort of waſps that have wrought this, it is a variety of architecture, but the principles of both the architects appear to be the ſame.

Fig. 10. repreſents the figure of one of theſe *American* neſts, which are made of a real paper like ours. This was communicated to me by M. *Vaillant*,

iant, a a the place where this neſt was ſuſpended to a little branch of the tree, b b b b the covering of the neſt, c c c c the aperture of the covering which has been enlarged, the waſps had made it leſs; DEF the laſt or lower cake, D part of the cake which is occupied by the cells, E part of this cake which has no cells and is poliſhed like the covering; F the hole through which the waſps paſs to enter the inſide of the neſt.

Fig. 11. is the ſame neſt of which I have taken away part of the covering, that we may ſee the diſpoſition of the whole cake, g g g g h h mark the edges of the places where the covering has been cut, and the covering itſelf at the ſame time, each K marks a cake, they are alſo each placed over-againſt the part where the hole is by which the waſps communicate with thoſe above or below.

Fig. 12. is a piece taken from *fig.* 11; l l mark one of the cakes, we ſee how it unites itſelf to the ſides of the covering; m m the inner ſurface of the covering which is very much poliſhed.

Fig. 13. is one of the cakes repreſented ſeparately, and ſeen above; n n n mark the upper part which is poliſhed, o o a part of the covering which has been preſerved, p p the place where the funnel begins, in which the hole or door of communication is perforated, F this hole or door of communication.

IV. *An experiment in gunnery.*

M. *de Reſſons* has ſhewed the academy the following experiment; he loaded a muſket with a ball, rammed in without any powder, and faſtened it to a poſt, becauſe it would be dangerous for a man to reſt it againſt his ſhoulder; he afterwards put ſome powder into a piſtol without a wad, and having

fitted


Fig. 1
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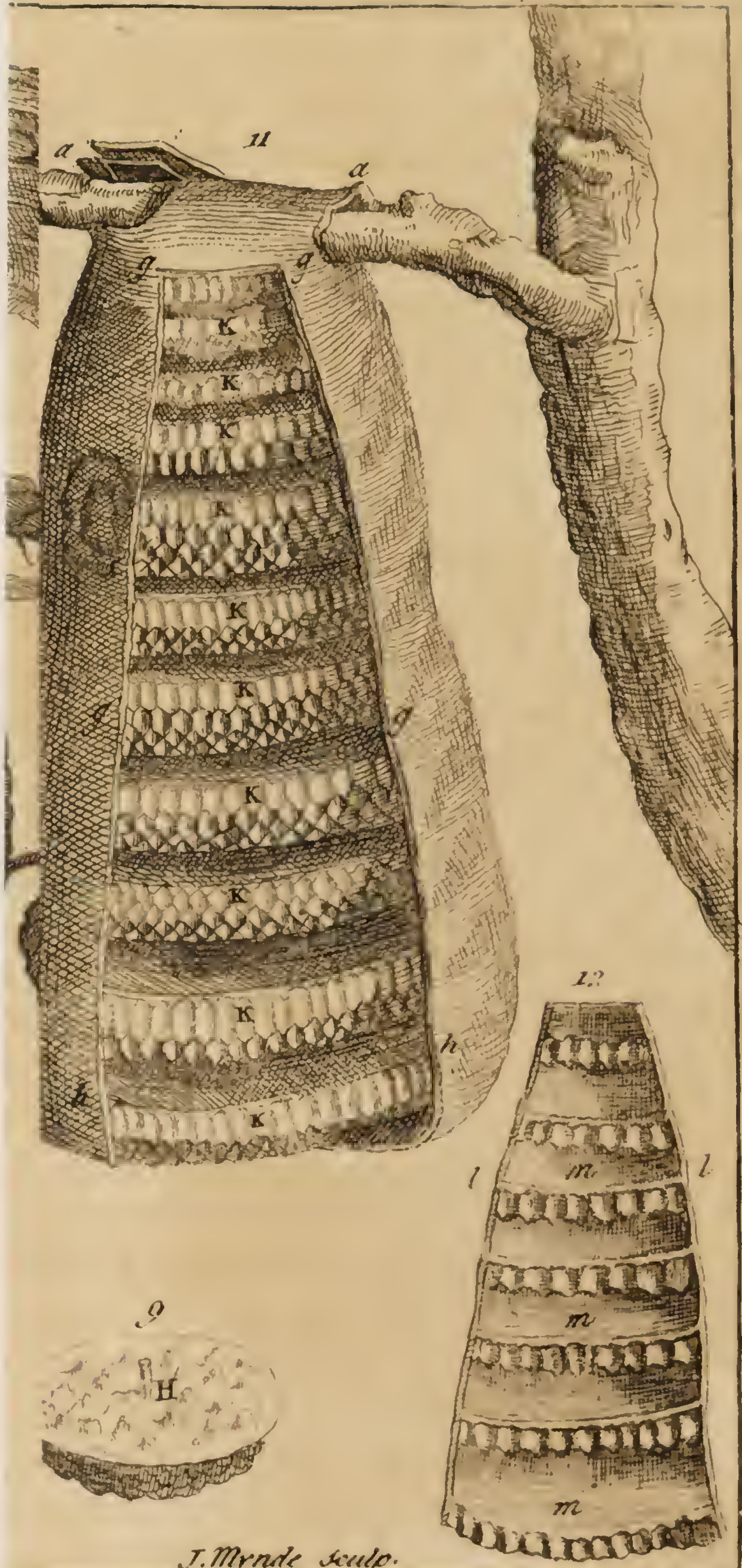
Scale of 10 Inches
J. Mynde sculp.



J. Mynde sculp.

fitted the mouth of the piſtol to the touch-hole of the muſket, ſo that the barrel of the piſtol and muſket made a right angle, he let it off; the inflamed powder of the piſtol, which entered at the touch-hole of the muſket, made the ball go out of it with ſo much violence, that it was able to go through a door at 15 paces diſtance, which exceeds the force of a piſtol. It appears by this, and by a greater report, that the force is increaſed, at leaſt it is certainly not diminiſhed, and it ſeemed that it ought to be ſo, ſince the effort of the powder, which can never be greater than when it follows a right line, was broken by the right angle of the piſtol with the muſket; and yet this turning away and the breaking did not weaken it, the kindled powder acted like a liquid, as water, which altho' it has been conducted through pipes diſpoſed according to different angles, makes always a throw of the ſame impetuoſity.

To ſucceed well in this experiment, we muſt chuſe a muſket that has a large touch-hole, and a piſtol that has a ſmall bore, we muſt alſo diſmount the plate of the muſket, not only becauſe it will be uſeleſs, but becauſe the mouth of the piſtol will be better applied to the touch-hole of the muſket.

V. *An extraordinary effect of thunder.*

The 14th of *April* 1718, in the night, there was in lower *Brittany* an extraordinary thunder, which M. *Deſlandes*, who was then at *Breſt*, has given a hiſtory of to the academy. It was preceeded by ſtorms and rains, which laſted almoſt without interuption for ſeveral days, at laſt came this night, between the 14th and 15th, which was ſpent almoſt entirely in very vivid and frequent lightnings, and almoſt without interruption. Some

ſailors who had put of from *Landernau*, in a little bark, dazzled by the continual fire, and not being able to govern it, let it go at random upon part of the coaſt which by good fortune was ſound. At four in the morning there were three claps of thunder ſo horrible, as to make the boldeſt tremble.

About the ſame hour, and in that ſpace of coaſt which extends between *Landernau* and *St. Paul de Leon*, the thunder fell upon 24 churches, and exactly upon thoſe churches where they rung the bells to diſperſe it, ſome neighbouring churches, where they did not ring the bells were ſaved. The people took it to be becauſe it was *Good Friday*, when they are not permitted to ring the bells. M. *Deſlandes* concludes that the bells which may diſperſe a diſtant thunder, facilitates the fall of that which is near, and almoſt vertical, becauſe the vibrations which it communicates to the air diſpoſes the clouds to diſperſe.

He had the curioſity to go to *Goueſnon*, a village about a league and half from *Breſt*, the church of which was entirely deſtroyed by this ſame thunder, they had ſeen three globes of fire, each of them 3 ½ feet in diameter, which being united again had taken their way towards the church with a very rapid courſe, this great globe of fire pierced it two feet above the ground, without breaking the glaſs of a great window not far from it, killed in an inſtant two perſons out of four who rung the bells, and made the walls and the roof of the church ſpring like a mine, ſo that the ſtones were ſcattered confuſedly about, ſome thrown to 26 toiſes, others plunged above two feet in the ground.

Of the two men who rung the bells at that inſtant, and who were not killed upon the ſpot, there remained one whom M. *Deſlandes* ſaw, he ſtill looked

looked wild, and could not ſpeak without trembling all over. They had digged him out of the ruins, ſenſeleſs, about four hours after. M. *Deſlandes* could get nothing elſe out of him, but that he ſaw the church at once all on fire, and that it fell at the ſame time. The companion of his fortune ſurvived the accident ſeven days, without any confuſion, and without complaining of any diſorder more than a violent drought which he could not quench.

VI. *Of an extraordinary lethargy.*

A councellor of the city of *Lauſanne* gave orders to one of his ſervants to ſet a preſs in order for receiving the grapes, when at once he loſt both his ſpeech and ſenſe. They thought he was fallen into an apoplectic fit, and they made uſe of the common remedies in that caſe. They were all to no purpoſe. The patient remained in a deep ſleep for ſome weeks, he opened his eyes from time to time, and even ſeemed to look with them, but they could not be aſſured of any mark that he really ſaw or that he had any ſenſe, they made him ſwallow ſome broth; all at once the ſleepineſs was accompanied with inquietude, the patient agitated himſelf and would riſe, but always without ſenſe; there afterwards came convulſions, which ended by the diſcharge of a pretty great quantity of matter, from his mouth and noſtrils. The lethargy only became more profound. The art of phyſick being at an end, an empyrick, who applied a great many cupping glaſſes to his head, had the honour of the cure; it was ſudden, his ſpeech and ſenſes returned in an inſtant, at the end of ſix months; by chance the ſame ſervant who had received the orders about the preſs, was at that moment near

his

his maſter, who aſked him an account of it, as if he had not had ſix months interval; his ſenſes returned to the ſame point where they had ceaſed, he lived 10 years after this accident in as good health as ever, and is dead of a common fever. The academy owe this account to M. *Crouzas* a famous profeſſor of mathematicks at *Lauſanne.*

VII. *Of a ſhower of ſand in the* Atlantic *ocean.*

The 6th of *April*, there fell in the *Atlantic* ocean, at 45° of northern latitude, and 322° 45′ of longitude, a ſhower of ſand which laſted from 10 at night, to 1 in the afternoon the next day. It was preceeded by a light like that which was ſeen at *Paris* the 30th of *March*, but of leſs duration. The winds were then at E S E. The captain of the veſſel, and all who were there, have atteſted this fact to F. *Feuilleé*, to whom they have given ſome of this rain, which it was eaſy to keep. He has ſhewn a little parcel of it to the academy; it is common ſand and very fine. The neareſt land to the place that has been determined is the *Iſle Royal*, which is 9 or 10 leagues diſtant from it. The ſhower of ſand muſt therefore have travelled at leaſt ſo far in the air.

VIII. *Of a great quantity of bones found in a rock.*

The royal academy of *Bourdeaux* have ſent to the regent, who has been ſo good to communicate part of them to the academy, ſome bones found in a rock, with a memoir of which we ſhall here only give an extract. In the pariſh of *Haux*, a

country

country between two ſeas, at half a league from the port of *Langoiran*, a point of a rock, eleven feet high was detached from a hill, which was before thirty feet high, and by its fall threw into the valley a great quantity of bones, or fragments of bones of animals, ſome of them petrified. It is beyond doubt that they are ſuch, but it is very difficult to determine to what animal they belong. The greateſt number of them are teeth, ſome perhaps of oxen, or horſes, but the greateſt part are too large or too thick to be ſo, without reckoning the difference of the figure. There are bones of thighs or legs, and alſo a fragment of the horns of a ſtag or elk. The whole was covered with common earth, and incloſed between two beds of rocks. We muſt neceſſarily imagine that ſome carcaſſes of animals having been thrown into a hollow rock and their fleſh having rotted, there was formed upon this maſs a rock of 11 feet high, which has required a long ſeries of ages.

If there had been only the bones of ſea animals in this heap, we have other well atteſted inundations which would eaſily explain this fact. If there were only bones of terreſtial animals, this place might perhaps have been ſome layſtall. If there were a mixture of ſea and tereſtial bones, the explanation would be more difficult.

The gentlemen of the academy of *Bourdeaux*, who have examined this matter like ſkilful philoſophers, would prove upon theſe bones, what M. *de Reaumur* has ſaid upon the origin of the *turquoiſes**. They have found in effect that a great number of fragments put to a very briſk fire, became of a very fine *turquoiſe* blue, that ſome little parts have taken the conſiſtence of them, and that being cut by a lapidary, they have taken the po-

* Page 91 of this volume.

liſh.

liſh. They have carried their curioſity farther, they have tried the experiment upon freſh bones, which have only turned black, except perhaps ſome little bits which approached a little to blue. From hence they conclude, with a good deal of probability, that bones to become turquoiſes have need to tarry long in the earth, and that the ſame matter which makes the black in the freſh bones, makes the blue in thoſe which have been buried a good while, becauſe they there ſlowly and by degrees acquire a certain maturity. It muſt not be forgot that the bones which viſibly belong to different animals have equally ſucceeded in becoming *turquoiſes.*

IX. *Of a toad found alive in the trunk of an elm.*

In the trunk of an elm of the ſize of a man, 3 or 4 feet above the root, and exactly in the middle there was found a toad, alive, of a middling ſize, lean, and poſſeſſing only its own little place. As ſoon as the wood was cleft, it came out, and eſcaped very faſt. There never was a ſounder elm, nor compoſed of more cloſe and connected parts; and the toad had not been able to enter into it by any part. The egg that had formed it muſt have got into the tree when it was young by ſome very particular accident. The animal had lived there without air, which is alſo very ſurpriſing, and was nouriſhed by the ſubſtance of the wood, and had not grown but in proportion as the tree grew. The fact is atteſted by M. *Hubert*, profeſſor of philoſophy at *Caen*, who writ it to M. *Varignon.*

X.

X. *Of a sort of fish supposed to be the* Galeus piscis *of* Rondeletius.

M. *Geoffroy*, the younger brother, shewed two fishes, which had been sent him from *Dieppe*, the one male and the other female. There came out of the female through a broken membrane 66 young ones alive. This fish was pretty much of the figure of an eel-pout, though we only know of fresh-water eel-pouts. M. *de Jussieu* thought that it might be the *galeus piscis* of *Rondeletius*, or the *blue-shark*.

XI. *Of an extraordinary animal brought from* Barbary.

The captain of the *Amazon* returned from *Senegal*, shewed M. *Deslandes* an animal taken by some hunters in *Portendic* or *Portandi* in *Barbary*, and which even the inhabitants of the country did not know. M. *Deslandes* called it *cani-apro-lupo-vulpes*, because of the resemblance it has to the different animals the names of which compose this odd name. It is almost two feet high, and about two and a half long, its hair is red mixed with black and yellow spots, pretty much like the bristles of the boar, these bristles are about 3 inches long all over the body, and near 5 upon the back, it erects them when it is angry, and then it resembles a *porcupine*; its head is something like that of a wolf but it is much larger at the top and decreases insensibly, its eyes are black and have a surprising livelyness, its ears are very long and always strait, his fore-legs are a little crooked, much thicker and higher than the hinder ones, and of the same colour with the legs of a tyger, there are

four toes well ſeparated to each paw with very ſhort nails a little hooked, its tail is long and thick and is like that of a fox, it runs faſt, eſpecially upon unequal ground, for upon an even ground it is apt to ſlide, as well becauſe it has no *talus*, as that its hinder legs are very weak in compariſon to its fore ones, it climbs eaſily, when it is warm it diffuſes a very ſtrong ſmell of muſk, though it is unquiet, and that the leaſt noiſe alarms it, yet it is pretty gentle, and lets one approach and careſs it without any uneaſineſs, it ſeldom cries and its cry is very ſhrill, it eats nothing but ſtinking meat; we might ſuſpect that this animal is a monſter, that is, born of a male and female of different ſpecies, we readily allow theſe ſorts of monſters to *Africa*, becauſe of the accidental meetings of a great number of different ſpecies of animals, upon the banks of the rivers, which are but few, and in a very hot climate, which excites them to mix indifferently, but we muſt not abuſe this idea, by making a monſter of every thing that we do not know.

XII. *Of the wild boars of* Africa.

M. *Deſlandes* has been informed by the officers of the ſame veſſel that the wild boars of *Africa* are very different from ours, they have no briſtles all over their body, their ſkin is ſmooth and even, but extremely hard, they have like the lions a very long and thick mane, which waves upon their neck, and hangs down to their feet, their tuſks are faſtened to the upper jaw, contrary to our boars, and they terminate in a very ſharp point. M. *Deſlandes* has ſeen thoſe of a young boar, killed by the hunters, they are round, exactly turned ſpirally, of as ſhining a white as ivory, and above a foot and half long.

XIII.

XIII. *On the velocities of bodies.*

Taking the common idea of gravity, and the ſyſtem of *Galileo*, if we ſuppoſe that two bodies of equal maſs which fall freely in a ſpace without reſiſtance, or in *vacuo*, have at a certain height, for example at 10 toiſes from the ſurface of the earth, an equal velocity, we ſee no difficulty in concluding that in all the other correſponding points of their fall, that is, in all the equal heights where they have been their velocities were equal, and that they will be ſo at all the equal heights where they ſhall be; this is certainly true, for in the preſent ſuppoſitions the velocities having been equal at a certain height, where the principles of inequality of the velocities at other heights ſhould be every thing elſe is equal, their maſſes, the forces that make them fall, the *initial* velocities which they have received from theſe forces in the firſt inſtant of their fall, ſince they fell freely, or by the action alone of gravity which is the ſame for both; in ſhort there is no medium that reſiſts them, or can alter their motion. The acceleration of the fall is therefore made in the ſame manner for both and as this acceleration follows a proportion, known from the height of the falls, it is impoſſible that if at a certain height it has produced the ſame velocities in both the bodies, it has not, or does not alſo produce the ſame velocity at all the equal heights. In a word all that conduces to the velocity being equal on both ſides, one ſingle point of equality of velocity in the two falls carries the equality throughout.

We generally imagine the action of gravity as conſtant, or the ſame at all the different diſtances from the centre of the earth, but if it is not ſo, and

ſhould be, for example, always greater according to any proportion whatſoever, as the diſtance from the centre of the earth ſhall be leſs, there is nothing changed in the propoſition juſt eſtabliſhed ſince this variation of the action of gravity will be the ſame for both the bodies.

If their maſſes are not equal, the propoſition is no longer of force, unleſs the gravity ſuppoſed variable according to the different diſtances from the centre of the earth, be not always proportionable to the maſſes, for then this will be a greater force which ſhall move a greater maſs more difficult to move, and this will be the ſame thing as if the maſſes and gravities were equal.

Gravity being a force which makes bodies tend to the centre of the earth, all other force which ſhall make them tend to any other points, will be in the ſame caſe, and we muſt reaſon upon it in the ſame manner.

It is not even neceſſary that the two bodies which tend toward the ſame point ſhould be impelled by the ſame force, they may be by two different forces, that is, the actions of which vary differently according to the diſtances of this point, but they muſt at the ſame diſtance have a conſtant proportion to each other, and then the propoſition ſtill ſubſiſts.

In ſhort it is not neceſſary that theſe forces ſhould be the only ones which act upon the two bodies, other forces may join them without diſturbing them at all. For example if the two bodies do not only fall by the action of gravity, but have been alſo both thrown from above downwards by a hand, and that at ſome equal height, they have the ſame velocity, they have always the ſame velocities at the ſame heights.

More-

Moreover, if being arrived at the end of their fall, they riſe again by any cauſe whatſoever, and at ſome equal height of this aſcenſion they have the ſame velocity, the propoſition is ſtill the ſame.

Although it appears plain enough, and perhaps ſufficiently proved from the little that we have ſaid yet geometrical ſtrictneſs would not be entirely content, and Meſſ. *Newton*, *Jean Bernoulli*, and *Heoman*, have given exact demonſtrations of it, but only in the moſt ſimple caſe; M. *Varignon* has elevated it to the greateſt univerſality poſſible, and has made it contain all that we have ſeen that could enter into it. For this reaſon he has taken one of the forms contained in the general theory of motions, either uniform or varied at diſcretion, and this form is ſo happily applied to his ſubject that by it he can compare the velocities of two bodies, taken at whatſoever unequal diſtances from the centre to which they tend, however different their initial velocities have been. We eaſily conceive that to find in a general manner that which reſults from the equality of velocity of two bodies at the ſame height, we muſt be able to compare their velocities at all the different heights.

M. *Varignon* ſuppoſes, that of two bodies if one falls by a right line, and the other by any curve whatſoever, that the two forces which impel them toward a common centre are variable, that they are different, or differently variable, that the common centre is only at a finite diſtance, all this as is eaſy to ſee, with a deſign of taking in at once all the particular caſes poſſible. We need only make the determinations as we pleaſe. For example, if the

two

two lines defcribed by the two bodies are ftraight, one vertical, and the other inclined to the horizon, if the two forces are the fame, if this only is conftant, laftly, if the common centre is infinitely diftant, which renders the directions of the force parallel between themfelves, we find our felves again in *Galileo*'s fyftem of gravity.

AN

AN
ABRIDGMENT
OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1719.

I. *Meteorological obſervations made at the royal obſervatory, during the courſe of the year* 1718; *by M.* de la Hire *the elder brother* *.

THE deſire of making ſome diſcoveries upon the origin of fountains, engaged my father in 1688 to make ſome experiments with relation to this ſubject. It alſo made him in 1689 begin to examine the height of water which fell in rain each year at the royal obſervatory. In 1696 he added to the obſervations of the rain thoſe of the barometer, thermometer, and of the winds, and has continued them to 1718. As I think theſe obſervations have been very well received by the publick, and as they may be of ſome uſe in phyſicks, I have taken care that the ſucceſſion of them may not be interrupted, as will be ſeen by the obſervations that I am going to relate.

Although the preceding year has ſeemed very dry and hot, it will however appear more ſo than we ſhould have believed, by the obſervations that we have made. The great drought has not prevented its having been very fruitful; and this is not from there having fallen a good deal of

* Jan. 7, 1719.

ſnow;

ſnow; for being reducéd into water, it gave but 4 $\frac{1}{2}$ lines; but it is that the rain fell in the exact time when the ground required it, as well for what it did produce, as for what it was to produce; ſo that we might ſay, that during this whole year, there has not been any unneceſſary rain; and this is all that we can deſire, to have a good harveſt in every thing.

Let us firſt examine the quantity of rain which fell during the courſe of the year, of which we ſhall only give the reſult of each month.

	Lin.		*Lin.*
Jan.	12 $\frac{1}{4}$	July	12 $\frac{3}{8}$
Feb.	8 $\frac{1}{2}$	Aug.	19 $\frac{1}{2}$
March	13 $\frac{1}{8}$	Sept.	9 $\frac{1}{8}$
April	22	Oct.	16 $\frac{2}{4}$
May	7	Novem.	4 $\frac{1}{2}$
June	25	Dec.	$\frac{3}{4}$

The ſum of the height of water is 157 lines $\frac{1}{4}$, or 13 inches 1 line $\frac{1}{4}$, which is much leſs than 18 inches 8 lines, which 30 years obſervation have determined to be the mean year.

The preceding year has been ſo dry, that theſe 30 years there has only been the year 1694, that was drier than this; and the difference was but 4 lines $\frac{1}{4}$.

By conſidering the reſult of each month, we ſee that it has rained almoſt one half more in the ſpring and ſummer, than in the winter and autumn, for there fell in the ſpring and ſummer 7 inches 11 lines of water, and in the autumn and winter 5 inches 2 lines.

The winds were generally pretty variable.

Upon the thermometer.

The thermometer, which is always the ſame, and has not changed its place, fell at the loweſt to 21 ½ parts the 10th of *Feb.* which is not a ſign of very great cold.

The heat has not followed the cold with relation to the mean ſtate, it has been much greater; for the thermometer roſe at the higheſt about ſun riſing to 70 parts the 22d of *Auguſt*, and during the ſame day about 3 in the afternoon to 82 parts, and not only on this day, but alſo on the 11th, the 21ſt and the 23d of the ſame month; ſo that if we take away 48, which is the mean ſtate, from 82, there will remain 34 for the difference of the mean ſtate from the greateſt heat; and if we take away 34 from 48, there will remain 14 for the point, where the thermometer ought to have fallen, if the cold had been equal to the heat in proportion to the mean ſtate, whereas it only fell to 21 ½ parts.

We may look upon the heat in 1718 as the greateſt that we have had at *Paris*; not but that this ſame thermometer has alſo riſen to 82 parts in 1706, 1707, and 1709, but it roſe to this point only once in each of theſe years, and in 1718 it roſe to it four different days, three of which followed one another; and it was this continued heat, although the ſame, which made us feel it ſo great.

It would ſeem by theſe experiments that the air ſhould be like water; that is, that it could only be ſuſceptible of a certain degree of heat, for we know that water which has boiled a certain time does not increaſe its heat, although we continue to make it boil. We ſhall endeavour to diſcover whether the air has this property, by

making ſeveral experiments which we have thought on, and we ſhall add them to ſeveral others which we have already made upon a matter that has a good deal of relation with this, in order to give them all together to the academy in another memoir.

If we have not yet tried the effect of a continued heat, at leaſt we have throughly felt that of a continued cold; for that in 1709 was a little leſs than that of 1716; yet the firſt has paſſed for the greateſt that has ever been, becauſe it returned ſeveral days to the ſame point; and in 1716 it was at the loweſt only during the night between the 21ſt and 22d of *January*. Theſe great heats and colds have given us occaſion to examine what are the greateſt mean colds, and the greateſt mean heats, and we have found for the greateſt mean cold, among thoſe of 23 years of exact obſervations which we have made, that the thermometer which we uſe muſt deſcend to 19 parts $\frac{2}{3}$, and that it muſt riſe to 75 parts $\frac{1}{3}$ in the greateſt mean heats. If we take the difference between the greateſt mean cold, and the greateſt mean heat, we ſhall have 55 parts $\frac{2}{3}$, the half of which being added to the greateſt mean cold, we ſhall have 47 parts $\frac{1}{2}$, which we may look upon as the mean ſtate; but this point is but one half of 48 parts diſtant, which is the point where this thermometer remains in the caves of the obſervatory, therefore we have reaſon to look upon the air of theſe caves as the mean ſtate.

Upon the barometer.

It is the ſame barometer which has always been uſed ſince we have made the obſervations of it, and which is always placed at the ſame height with regard to the level of the river; it fell the

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loweſt to 27 inches, 0 lines, 0 points, the 11th of *January*, with a ſouth wind, and roſe the higheſt to 28 inches, 4 lines, 2 points, the 15th of *February*, with calm weather, and afterward with a gentle north-eaſt wind.

Theſe two obſervations of the greateſt ſinking, and of the greateſt elevation of the quickſilver in the barometer, agree perfectly with what we have thought for ſome time, that the winds from the north raiſe the atmoſphere, and thoſe from the ſouth lower it.

To ſupport this opinion we may alſo relate, that we have made very exact obſervations upon the barometer theſe 23 years, and that in this number of years there are 17 where the wind was toward the north when the barometer was the higheſt; and of the other 6 there was ſometimes no wind near the earth; not but that there might be ſome in the air, where the wind had changed all at once, and had not yet made itſelf ſenſible to the barometer, or there was only a wind near the earth, of but ſmall extent, and not capable of changing the atmoſphere. It is almoſt the ſame thing for the greateſt ſinkings; for of theſe 23 years there were 15 where the wind was toward the ſouth, when the barometer was at the loweſt, and in the others it might happen that the wind was in the ſame circumſtances as thoſe that we have related in the greateſt elevations.

We have only given attention to the greateſt alterations of the barometer, becauſe we were perſuaded that it would be in theſe points that the cauſe of it would be moſt marked, and conſequently more eaſy to find out.

The above remark appears to have ſo much relation to the others that follow, that they ſeem to us to proceed from the ſame cauſe; and thus

we muſt ſpeak of them all before we inquire after the reaſon of them.

During the 23 years exact obſervations, there were 21 when the barometer was at the higheſt, with a wind that was but moderate at the higheſt, and which was toward the north.

It is not only in theſe points of the greateſt elevation, that there is hardly any wind, and that towards the north, but there have not been any years when it did not happen two or three times that the quickſilver has been above 28 inches for 8 or 10 days together with a feeble north wind.

As to the greateſt ſinkings which have happened in 23 years, there have been 13 when the wind was leaſt ſtrong, and toward the ſouth.

Continuing to examine the obſervations upon the barometer, we have alſo remarked that the greateſt changes in the barometer only happen in the two firſt aud laſt months in the year, and chiefly in the firſt and laſt; ſince of the 23 years there were 20 when the barometer was at the higheſt in the two firſt and laſt months of the year, and 17 when it was at the loweſt during theſe ſame months; and in theſe two numbers of years, of 20 and 17, there were 12 in the firſt, and 11 in the laſt when it was at the higheſt, and at the loweſt in the months of *January* and *December*.

It does not however appear that there can happen any great changes in the atmoſphere during the beginning and the end of the year; all that we obſerve at that time, is feeling a preparation to cold, the cold itſelf, and a ceſſation of cold; we might therefore ſay that the air, becoming more or leſs cold, or more or leſs condenſed, produces in the barometer its greateſt changes. To ſupport this conjecture, we may probably ſuppoſe that

that in the north the cold is not continued during a whole winter, when once it has begun, and that it is there, as in this country, ſometimes greater and ſometimes leſs; but by degrees as the cold diminiſhes the air dilates itſelf conſiderably, becauſe it is ſtrongly condenſed by the cold; but this dilated air making an effort on all ſides, and finding that of the ſouth the weakeſt of all, it makes its whole effect on that ſide, and makes us feel a north wind, which cannot be violent nor laſt long, becauſe the heat or the dilated air which produces it, has not been either conſiderable or long, becauſe of the country and of the ſeaſon; and as this wind is much cooler than the air here, it will condenſe it, and fill the place which it has made it abandon in proportion as it arrives there; and the condenſation will be ſo much the more conſiderable as the wind that comes from the north ſhall be colder with regard to our air. There will be therefore in the ſame ſpace many more particles of air than there were before, which will preſs upon the quickſilver, and raiſe it very high in the tube of the barometer. It is plain therefore that we ſhall not be able to find any but the cold northern winds, though very feeble, that can produce this effect in the barometer.

By this explication of the greateſt heights of the barometer during the cold, we ſhall give a very good reaſon for the great fogs that almoſt always accompany them; that the cold renders the aqueous parts, which ſwim in the air, viſible; and this air becoming more heavy, ſuſtains them without their being able to make any effect upon the barometer, contrary to the opinion of ſome perſons, who attribute to them the cauſe of the greateſt heights, ſince at moſt they can only hold

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the place of a like bulk of liquid in which they ſwim, according to the rule of floating bodies.

On the contrary, the ſouth winds at the end of autumn and the beginning of the winter, coming from a hot country, where the air is dilated, and arriving here in this, where it is much colder or more condenſed, they dilate it and impreſs on it a motion to go toward the north; but as it finds a good deal of difficulty in it, becauſe the air there is ſtrongly condenſed, it muſt have a great force to overcome this obſtacle: for this reaſon it is neceſſary that the ſouth wind ſhould be violent; but this wind cannot be violent, if it does not carry away with it a good deal, not only of the air which it had dilated, and which could not remain there any longer, but alſo of that which cannot reſiſt the violence of the motion, ſo that there are much fewer particles of air in the ſame ſpace than there were before, and conſequently the quickſilver muſt ſink conſiderably.

Although, as we have juſt ſeen, we cannot do without the elevations and depreſſions of the atmoſphere, to aſſign the reaſon of the great heights of the quickſilver with a weak north wind, and of its great fallings with a violent ſouth wind at the beginning and end of each year, yet we dare not advance as a certain fact, that the atmoſphere never contributes, by changing its height to any of theſe effects that we have related; not but that we are fully perſuaded that whatſoever wind there may be near the earth, it can never alter the figure that the motion of the earth upon its *axis* in 24 hours has given to the ſurface of the atmoſphere.

We ſhall take care hereafter to attend to the remarks that we ſhall give, if they agree with the obſervations that we make.

Of the declination of the needle.

We have obſerved the declination of the needle the 30th and 31ſt of *December* 1718, in pretty calm weather, the 31ſt with a needle of 13 inches and an half in the ſtone compaſs, and we found it 11 degrees 30 minutes to the north-weſt, as we took notice that the obſervations of the 30th and 31ſt of *December*, gave too great a difference from 1717 to 1718, we were determined to make another the 7th of *January* 1719, in pretty calm weather, by animating the needle beforehand, for fear that any accident had happened to it, but it always gave the ſame declination.

We have alſo obſerved the declination of the needle at the ſame place the 7th of *January* 1719, with a needle of 8 inches, and found it to be 12 degrees 20 minutes, although it was animated at the ſame time with that of 13 inches and an half, and with the ſame ſtone.

II. *Reflections upon ſeveral obſervations concerning the nature of the* gypſum, *by M.* de Juſſieu *; *tranſlated by Mr.* Chambers.

Gypſum in the general is a ſoft friable ſtone, void of taſte and ſmell, and eaſy to be calcined by the ſmalleſt fire. The *calx* or lime ariſing from this calcination is called plaſter or parget, and popularly plaſter of *Paris*.

Being mixed with water, and thus kneaded into a paſte, they faſhion it, while ſoft, into what figure they pleaſe, which hardening in a ſhort time after, arrives at the conſiſtence of a ſtone.

* March 24, 1719.

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The fragments of this ſtone are incapable by any calcination to be reduced a ſecond time into *calx*, or to be uſed again in the compoſition of a paſte like the former.

There are three ſpecies of *gypſum.* The firſt found in blocks and large maſſes like common ſtones dug out of quarries, and only differing therefrom by its greater ſoftneſs and eaſineſs of being calcined, it is called accordingly plaſter-ſtone, or parget-ſtone; and of this does the whole mountain of *Montmartre* conſiſt.

The ſecond ſpecies of *gypſum* is diſpoſed on leaves or *laminæ*, like talc or the *lapis ſelenites*, being tranſparent too like them, and only differing from them in this, that thoſe others are not calcinable by the fire. Its tranſparency has alſo given it the name of *lapis ſpecularis*, and in *French pierre à miroir*; *q. d.* looking-glaſs ſtone. This kind we find of two different figures in the neighbourhood of *Paris*, both of them deſcribed by M. *de la Hire*; and in other countries there are other figures.

The third kind of *gypſum* has its parts ranged needlewiſe, almoſt like antimony, or in ſilvered *ſtriæ* or fibres like thoſe of plume allum, which would occaſion them to be frequently confounded if their difference was not ſufficiently noted by the acidity and ſtipticity peculiar to this laſt, and its facility of diſſolving in water.

The nature of *gypſum* has hitherto been but little examined, whether on account of its being too common, and therefore neglected, or by reaſon it has been ſuppoſed equally impenetrable with that of lime itſelf. However, as the attending even to trite *phænomena* frequently leads us to the diſcovery of the moſt obſcure cauſes, I flatter myſelf with having attained a more compleat knowledge

knowledge of this mineral than has hitherto been had, from ſome reflections which I have made on the cauſes of certain cryſtallizations in the copper mines near *St. Bel* in the *Lionnoiſe*, and the caverns of the mountain *Almaſaron* in the kingdom of *Murcia*. My obſervations whereon, made by the naked eye, having been ſince confirmed by an exact examination with the microſcope, have convinced me,

1ſt, That as there are ſaline, vitrious, ſulphurous, and metallic ſubſtances mixed with certain bodies, there are alſo gypſous ones intermixed in the like manner.

2dly, That theſe gypſous particles which enter the compoſition of diverſe mixts, have a determinate figure peculiar to them, which they ſtill retain, whatever alteration thoſe mixts happen to undergo either by calcination, trituration, or the admixture of other bodies employed to deſtroy them.

3dly, That this figure is ſo conſtant in ſuch gypſous parts, that when they abound in any ſubject, the other parts mixed along with them, whether they be ſaline or metalline, ſuit their diſpoſition to the figure of thoſe gypſous parts.

4thly, That thoſe gypſous parts, which one would not have ſuſpected in the compoſition of a mixt, or one would have thought deſtroyed therein, after they had been diſcovered, will frequently become ſenſible again, re-appear under their uſual forms, and reſume their firſt qualities, either by a natural effect, or by ſome aſſiſtance of art.

All which conſequences are fairly deducible from the following obſervations.

In the copper mine near *St. Bel* I found the ſtone which uſually covers the veins of ore, and adheres thereto, compoſed of diverſe *laminæ* laid one ovrr

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another of a ſilver colour on the outſide, and browniſh within.

The colour of this inſide, with the quantity of little ſhining ſpecks, and the heavineſs of the ſtone, led the workmen to imagine it full of metalline parts, and accordingly they throw it among the reſt into the furnace prepared for the calcination of the pieces of ore.

Now it is evident the fire uſed in this calcination, being to penetrate and turn the ſtones quite red, muſt vaſtly ſurpaſs the force of that commonly employed for the calcination of *gypſum*, and conſequently if the matter which covers the ſilvered ſurface of moſt of theſe ſtones be a *gypſum*, this *gypſum* has had more fire than was neceſſary to calcine and reduce it into plaſter.

After this calcination they caſt the pieces of ſtone, which are now almoſt as red as colcothar into vats, and pouring water on the ſame, that liquor imbibes their metalline, vitriolic, and gypſous parts, and is at length ſeparated therefrom by running from ſuch vats into a baſon, in the middle whereof are placed ſeveral pieces of old iron, which ſeem here to be transformed into copper.

During this *metamorphoſis* a thick ſmoak is ſeen like a white cloud, which ſpreads over the edges of the baſon, and even a foot beyond. This cloud gradually diſſolves again, and I have found that when it was fallen, not only the edges of the baſon, but even the ground all around, were covered with a multitude of little white flat cryſtals about half an inch long, and a line broad, nearly of the figure of a parallelipiped, perfectly tranſparent, inſipid, and uniform, which the cloud had let fall.

Theſe cryſtals, by repeating the operations, become collected in ſufficient quantity to form a

maſs

maſs reſembling a ſtone, which by its figure and colour may be compared to thoſe large ſtones of crude tartar brought from *Marſeilles* and *Montpelier*, and which in reality are no other than true *gypſum*, the cryſtals whereof they are formed being found inſipid, tranſparent, of a parallelipiped figure, not diſſoluble by water, and eaſily calcinable by fire, which are the preciſe characteriſticks of *gypſum*.

In the courſe of this obſervation, I perſuade myſelf to have ſufficiently ſhewn that the ſtone from whence this copper and vitriol are drawn, bears ſome reſemblance to that of *gypſum*, that it has undergone a calcination more than ſufficient to turn the gypſous part of its compoſition into plaſter, and that by the alteration this plaſter undergoes in its diſſolution in water, it becomes a matter like dried plaſter, yet contrary to the nature of other dried plaſter, which is not calcinable nor capable of being uſed anew. We find that this plaſter, though always ſuppoſed to be diſanimated, will ſtill yield cryſtals whoſe particles are perfectly like thoſe of the ordinary *gypſum*; and I have even found by experiment, that the maſs which ſuſtains theſe cryſtals, and which to the naked eye appears only a maſs of hardened plaſter, is ſuſceptible like the cryſtals themſelves of a new calcination, as eaſily as *gypſum*, and capable like it of being reduced into effective plaſter.

As to the reddiſh colour perceived in this maſs of cryſtals, it can only be owing to the duſt of colcothar, which upon ſtirring it ſpreads all around the vats.

This diſcovery has given me occaſion to examine the *phænomena* preſented by vitriol in the torture it undergoes to make it yield its ſpirit,

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and the proceſs afterwards uſed to compoſe the ſalt called *Glauberi.*

In the firſt of theſe operations I find a great conformity between the ſpecies of colcothar produced by calcination of the ſtones of the mine of *St. Bel*, with the vitriol put in a retort, in order to extract its ſpirit, ſince, as I have already ſhewn, this colcothar of *St. Bel*, when thrown into a proportionable quantity of water, diſſolves into a blueiſh ſtiptic and corroſive liquor, from whence, beſide the metalline matter ſeparated from it, there likewiſe ariſes by effervеſcence a gypſous matter, which reſolves on the edges of the baſon into parallelipiped cryſtals. So in the compoſition of the *ſal Glauberi*, whereof Meſſrs. *Boulduc* and *Geoffroy* have given us the proceſs at large, when the oil or ſpirit of vitriol is ſufficiently diffuſed through a proportionable quantity of water, there riſes on the water in the time of its evaporation, a white ſilvered ſcaly cream or froth, which being drawn from the water and dried, is found inſipid to the taſte, indiſſoluble in water, and eaſy to be calcined by the gentleſt fire, qualities which render it in all reſpects like the *gypſum* taken from the edges of the baſon, which receives the ſolution of colcothar from the mine of *St. Bel.*

The only difference obſervable between this laſt *gypſum* and that of *St. Bel*, is that the former ſhews itſelf under a ſcaly figure, whereas the latter appears in form of cryſtals; but this difference is leſs ſurpriſing, as the ſcaly and ſilvered figure of the *gypſum*, coming from vitriol, agrees with that of the ſilvered ſurface found on moſt of the ſtones calcined at *St. Bel* for the procuring of copper.

And though the experiment of Meſſrs. *Boulduc* and *Geoffroy* were not made with vitriol procured from

from the mine of *St. Bel*, it is probable if that were uſed it would produce the ſame effects, and the rather, as that procured from the colcothar of this mine, is perfectly like common copperas, and was originally mixed with *gypſum*.

My ſecond obſervation was made in the allum mines of *Almaſaron*, a town in *Murcia*. The mount which covers them is very high, and the earth it is formed of full of veins of two kinds of ſtones, one whereof by its colour reſembles the magnet more than any other ſtone, whereas the other both in figure and colour reſembles fragments of the hardened and diſanimated plaſter.

The middle of the mountain preſents apertures in divers parts of it, which lead to ſubterraneous grottos, into ſome whereof I went down, and by the favour of flambeaux could ſee vaults and partition walls inveſted with a whitiſh velveting, much like the hoar which ſometimes covers the walls in froſty weather, with this difference, that the hoar abovementioned was more abundant in ſome parts than in others, and ſtood out prominant from the wall ſome two or three inches, reſembling ſo many bunches of white hair. Tearing ſome of them off, and coming to obſerve them in the open day, I found that the threads whereof they conſiſted were friable, acid, and ſtiptic to the taſte, ſoluble in water, and ſilvered to the eye, which made me take them for a plume allum, though the inhabitants only conceive them as a common allum.

Preſenting a piece of this aluminous ſubſtance to the flambeaux, part of it reddened and turned into colcothar, ſtill reſerving its ſtipticity, while the other remaining white turned into a white powder like that ariſing from *gypſum* when calcined and reduced to plaſter.

Comparing

Comparing this matter with that I had obſerved in the veins of the mountain of *Almaſaron*, I came into the opinion that this ſubſtance ſticking on the *parietes* of the caverns, is no other than a revivification of the third ſpecies of the *gypſum*; in effect, it has the preciſe colour thereof, its texture is withal diſpoſed into parallel fibres, which render it ſtriated, and part of its ſubſtance turns to plaſter.

If therefore I did not find on the top of this mountain a native *gypſum* of the third kind, at leaſt I may be allowed to aſſert that it exiſts there, from the obſervation of thoſe white ſtones, and from the plenty of this kind of ſtriated *gypſum* found in ſeveral parts of *Spain*.

It cannot be doubted but that the gypſous matter predominates in this plume allum, ſince by a bare expoſing of it to the fire, it eaſily ſeparates from the ſaline ſubſtance. The mixture is even perfectly like that found in the mine of *St. Bel*, where the mixture does not ſeparate till the ſame acid ſalt, wherewith it was united, becomes joined by art to certain metalline parts extraneous to this mixt; whereas in that of *Almaſaron*, having no extraneous metalline ſubſtance to incorporate withal, it remains united with this third kind of *gypſum*.

But what ſtill better eſtabliſhes the relation between this ſubſtance and that of *St. Bel*, is the uniformity of the matters found in thoſe two places; ſince as the ſtone of *St. Bel*, after calcination, appears in form of a true colcothar: ſo at the foot of the mountain *Almaſaron* we meet with a browniſh earth, called by the natives *almagra*, which is no other than a native colcothar. And as a mixture of the acid ſalt of vitriol with the iron it is poured on, is capable of forming green cryſtals of copperas,

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ſo in the grottos of *Almaſaron* the mixture of a larger quantity of parts of iron with the acid ſalt of plume allum, produces green cryſtals of copperas covered with fibres of plume allum.

The only difference between theſe two obſervations, is in the manner wherein the *gypſum* of the mine of *St. Bel* is diſengaged from the ſubſtances it was united withal, which is here done by art, whereas the ſeparation of the more imperfect *gypſum* at *Almaſaron* is done naturally.

The two obſervations are happily confirmed by certain others which I made by a microſcope on the four different ſubſtances of *gypſum*, plaſter, hardened plaſter, and the cryſtallized *gypſum* of *St. Bel* and *Almaſaron*.

I took a piece of *gypſum* from the quarry of *Montmartre*, and having pulverized it, obſerved that all the grains of the powder into which it was firſt reduced, were very tranſparent; and that upon a further levigation they appeared of a long nervous, and parallelipipedal figure, though ſuch as were ſmaller than the reſt, were almoſt ſpherical; it is eaſy to judge that theſe parallelipipeds are the pure *gypſum*, and that the little ſpherules are by no means the ruins of the parallelipipeds, ſince by the microſcope the fineſt leaves of *gypſum* appear ſtriated; and the interſtices between the *ſtriæ* filled with theſe ſpheroids.

Viewing by the ſame microſcope another parcel of powder of the ſame *gypſum* calcined and turned to plaſter, I could perceive each parallelipiped grain ſtrewed irregularly over with theſe ſpherules. At the ſame time I obſerved that the ſpherules were more or leſs numerous according to the time which the calcined mixt had been expoſed to the air; and that the ſame ſpherules joined themſelves very readily to the moiſture of the air, ſo that they

they would not only be carried away very nimbly like all volatile ſalts, but that when incorporated with this moiſture, they aſſumed a flat oval figure, which makes me conſider theſe ſpherules as the ſaline particles of the mixt.

By means of the ſame microſcope I found the ſame parallelipiped figure in ſeveral particles of the powder of hardened or diſanimated plaſter, with this difference, that they were intermixed with other particles of a figure different from the former, and from that of the ſpherules in the two preceding obſervations.

This ſeparation of the gypſous parts, which are the parallelipipeds from the ſaline ones, which are the ſpherules; and this mixture of other particles different both from the parallelipipeds and ſpherules, can only ariſe from the addition of certain terreſtrial parts introduced by the water into the plaſter in beating it, to which the ſaline particles have joined themſelves as intimately as they had before done with the parallelipipeds. This ſeems to be the cauſe of the difference between the powder of calcined *gypſum* and that of hardened plaſter, the former of which being mixed with water, has the property of turning into a manageable paſte, which in a little time becomes as ſolid as ſtone, whereas the latter will never incorporate thoroughly by means of water, and even remains much longer without drying, becauſe in the former the ſaline parts ſticking on the ſurface of the parallelipipeds, bind them in ſome ſort together, which is further ſtrengthened by the terreſtrial particles depoſited by the water in the interſtices between the globules; whereas in the powder of the hardened plaſter, the parallelipipeds having none of theſe ſpherules on their ſurface, on account that they are faſtened to the terreſtrial particles

ticles of the water, they can no longer unite together as before; and the ſame alſo befalls beaten plaſter, which having been expoſed ſometime to the air, has evaporated its moiſture, and to *gypſum* itſelf, by lying too long in the rain and ſun.

Laſtly, I have examined the gypſous cryſtals of *St. Bel* by the microſcope, having firſt turned them into powder, each grain whereof appeared of a figure much like that of the plaſter of *Montmartre.*

The ſilver cream gathered by Meſſrs. *Boulduc* and *Geoffroy* in making their *ſal Glauberi*, and which we have aſſerted to be gypſous, appear by the microſcope to be compoſed of almoſt as many parallelipipeds as grains.

The native plaſter which I gathered on the mountain *Almaſaron*, appeared when turned to powder nearly of the ſame form, excepting that the parallelipipeds were a good deal leſs perfect.

But I was ſoon after let into the cauſe of the different figure of this plaſter from that of *Montmartre*, upon viewing by the ſame microſcope thoſe tufts of plume allum which I had gathered in the ſubterraneous vaults of *Almaſaron*, for I found that the gypſous parts of this allum, in lieu of being parallelipipeds, appeared as it were of a cylindrical figure, and not edged as thoſe of common *gypſum*, which convinced me ſtill farther, that this plume allum was partly formed of the *gypſum* reduced into plaſter on the top of the mountain, and cryſtallized in its caverns.

Theſe laſt obſervations made by the microſcope, gave me the more pleaſure, as they proved perfectly conformable to thoſe made by M. *Lewenhoeck* on the firſt ſpecies of this mixt.

Laſtly, From theſe ſeveral obſervations, which are all conformable to each other, I conceive the

four consequences proposed at the beginning of this memoir, as abundantly proved, *viz.*

That of the mixture of gypsous parts in divers bodies.

Of the constant figure of such gypsous particles.

Of the communication of this their figure to the mineral substances they are combined with.

And of the revivification of the same gypsous particles in bodies wherein they seemed to have been destroyed.

And if these principles which unfold the nature of *gypsum* may pass for certain, they will lead us to a more perfect knowledge than hitherto we have had of those which compose other mineral bodies.

They will teach us, for instance, that if notwithstanding the utmost torture we can give to water, salt, glass, sulphur, *gypsum*, or metals, those substances still resume their first form, they must be composed of particles whose figure is peculiar to them, and incapable of being destroyed.

From which consequence arises this other, that as we are not to admit any destruction of metals and minerals, other than the *metamorphoses* which arise by the disunion of their most intimate parts, so we are to admit no other reproductions or revivifications besides the re-union of those same intimate parts, which before were separated and intirely disguised by the addition of other foreign substances.

III. *On*

III. *On the method of procuring mercury from the mines of* Almaden *in* Spain, *and the nature of the diſeaſes of thoſe employed therein, by M.* de Juſſieu; *tranſlated by Mr.* Chambers.

The uſe of mercury, called alſo quickſilver in the arts, and eſpecially in the purification of gold and ſilver, has rendered princes in whoſe domains this mineral is found, highly ſolicitous about the means of multiplying the produce thereof; ſo that an inquiry into the methods by which this is effected, will be found equally curious and uſeful.

For this reaſon, to make all the advantages I could of my late journey into *Spain*, enjoined me by the king and the duke of *Orleans* for the improvement of *botany*, I apprehended, that having an opportunity to obſerve the works in a mine which paſſes for the moſt antient as well as the richeſt in *Europe*, the intelligence which I gathered therefrom, by his catholick majeſty's permiſſion, might not be uſeleſs in our own country, in caſe of the diſcovery of any mines of this kind.

That of *Almaden*, whereof I am now to ſpeak, takes its name from a town in a little province of *Spain* called *la Manca*, on the frontiers of *Eſtramadura* to the weſt, and ſurrounded on the ſouth with ſeveral mountains belonging to the *Sierra Morena*, or black mountain.

The town is ſituate on the top of a mountain on the declivity, as alſo at the foot whereof, towards the ſouth, there are five ſeveral apertures, which by ſo many ſubterraneous paths, lead to the places where the cinnabar is dug.

There are none of thoſe external marks without the mine, either in the colour of the earth, the rubbiſh at its entrance, or the ſmell exhaling from it; which uſually indicate this mineral.

Of the five apertures, two are ſurrounded with walls which form a kind of large courts, in one whereof are forges for refitting and mending of the inſtruments neceſſary in digging the mines, as alſo ſheds, under which the neceſſary timber works are framed; in the other, which is a few paces diſtant from the former, upon the aſcent; towards the town is a well dug pretty deep, which lets light in one of the moſt conſiderable branches of the mine; it alſo ſerves to let down beams, props, *&c.* for ſhoring and ſuſtaining the earth, and is of ſome further uſe for drawing up large lumps of the mineral, which it would be troubleſome to convey as far as the firſt aperture, by which the workmen enter.

In this ſecond court is a long piece of wood, placed horizontally, and ſuſtained by its middle on a pivot raiſed about half a foot from the ground, wherewith they weigh the large lumps of mineral drawn up by this well, and according to the weight thereof pay the workmen their wages.

The third aperture, which is a few paces from the former, but almoſt in the ſame line, is incloſed by a building which ſerves as a priſon to the malefactors condemned to the mines; by this they deſcend and aſcend again from the mine.

The fourth, which is on the higheſt part of the mountain, and within the town itſelf, only ſerves for going down into the mines on extraordinary occaſions.

And laſtly, the fifth, which is within the ſame incloſure as the firſt, is always ſhut, being never to be opened except for the benefit of rheumatic

people,

people, who enter by it into a place where a warm vapour arifes, which gives them a gentle fweat.

What is moft extraordinary in this mine, is the contrivance of the places, by means whereof the branches which lead to the places given over, are gradually filled up with the earth procured from thofe actually in digging, fo that not only the carriage of earth to a great diftance is prevented, but alfo thofe fhudderings and fallings in which happen but too often in fubterraneous places.

As to the guts or paffages leading to the works, their ftructure is very neat, they are about feven foot high, and four or five wide, the roofs thereof being fuftained by oaken planks laid over two pofts of the fame wood, fixed againft the two fides of the paffage.

The earth in thefe paffages is not fo moift as is commonly found in fuch places, for befide that the miners dig at the drain at the bottom of one of the fides, which being continued through the aperture of the mine, carries the water into a well dug for the purpofe; they take care to cover both this drain and the middle of the paffage with boards faftened end to end, which lead to the place of working. The fmoothnefs of the plane formed by thefe boards facilitates the paffage of a fort of little carts with four wheels, loaden with three or four bafkets full of pieces of ore, and pufhed along by the workmen.

The veins which appear at the bottom of the place where the miners are employed, are of three forts. The moft common is a pure rock of a greyifh colour on the outfide, and intermixed with reddifh, whitifh, and cryftalline ftains within.

This

This first contains a second, consisting of the reddish parts among the former, and those which come nearest the colour of *minium*.

Lastly, the third, which is of a more compact substance, very heavy, hard, and grained like freestone, is of a brick-coloured red, intermixed with a multitude of little silvered sparks.

Among these three sorts of mineral veins, which are the only valuable ones, are found several other kinds of stone of a greyish slate-colour, and two kinds of fatty unctuous earth, which they throw away.

Having picked out the pieces of the three sorts of veins above-mentioned, they convey them into an inclosure at the extremity of the town, on the top of the mountain to the western side, wherein are built several furnaces for the separation of the mercury.

These furnaces, which are joined by two and two, form an oblong building about 12 foot high, resembling on their inside, which is only 4 foot and a half broad, our common lime-kilns.

The hearth, which is about five foot high, is for laying the wood on, and the space which from the grate to the dome is about seven foot, serves to contain the pieces of the three sorts of stone above-mentioned; those of the first, which are about the size of our rough stones, are put immediately upon the hearth, which is of brick, by a door, which opens aside of this hearth; those of the third, which are somewhat less, are thrust into the space over the former; and lastly, those of the second, which cannot be put in by the door of the grate, are placed by the aperture of the dome; and in regard these last are the smallest, by reason their vein easily breaks, they mix them up with clay, and form a sort of square loaves, which

which are to be dried ere they put them in the furnace.

The furnace being thus filled within a foot and an half, which is left for the vapour to circulate by, and the door of the grate as well as the dome being closed with brick, they light a wood fire on the hearth, the smoak whereof goes out by a tube contrived in the thickness of the wall, and continued like a chimney two or three foot beyond the ridge of the building.

The hind part of the furnace, which is that opposite to the aperture of the hearth, rests all but a foot and an half of its height against a terras; and this part, which exceeds the terras, is pierced in its whole compass with 16 spiracles each, 7 inches in diameter, and raised in the same horizontal line: this terras, which is only five fathoms long, is terminated by another little building which faces the hind part of the furnaces, and its floor being paved, descends on each side, by which it touches those opposite buildings in an easy declivity, which forms a gutter in the middle of this space.

The use of this terras is to sustain several aludels or earthen vessels pierced at both ends, they are half a foot in diameter, and two foot long; and from the 16 spiracles of the two furnaces, to the same number of apertures made in the foot of the front wall of the little building opposite to these furnaces, form lines of communication like large strings of beads.

By means of these aludels, the sulphurs and mercurial vapours raised from the ore by a violent fire, during 13 or 14 hours, are conveyed to this little opposite building, and do not escape by the four chimney tubes which open therein, till they have

have depoſited their heavieſt parts, which are the mercury revivified in theſe aludels.

They let the furnaces cool for three days, after which the aludels are unluted, and the mercury poured out of them into a ſquare chamber, whoſe ſides run aſlope and terminate in a little ciſtern placed in the middle thereof.

In running from the extremities of this chamber into the ciſtern, the mercury purges itſelf of a black duſt which is left ſticking to the bottom of the chamber, to be ſwept away by the women from time to time.

The uſe of the gutter in the terras, is to collect all the mercury which might have eſcaped through the luting of the aludels, and upon the ſtirring and ſhifting them.

And the four chambers into which the little building that terminates the terras is diſtributed, ſerve as ſo many receivers, where the ſmoak, by the ſtay it makes there, depoſits ſtill more mercury, which they find here as well as in the aludels. There is an entrance into each of theſe chambers by a window, which they cloſe very exactly with luted bricks in the time of the operation.

The quantity of mercury which one furnace full of pieces of the three kinds of this ore will afford by one coction, is very conſiderable, amounting at leaſt to 25 quintals, ſometimes to 30, and ſometimes even to 60 quintals of revivified mercury.

The mercury produced at each coction, is carried into a magazine built in the ſame incloſure, where it is preſerved in ſheep-ſkin bags ſuſpended over earthen veſſels, till they ſend it to *Mexico.* In the year 1717 there were 25000 quintals of this mineral remaining in the magazine after a much

much greater quantity thereof had been ſent to *Seville*.

After this I might ſubjoin ſeveral explanations upon the quality of different kinds of cinnabar treated of by *Pliny*, lib. 33. cap. 7. and ſhew that the cinnabar mine in *Spain*, whereof he ſays the *Romans* were ſo jealous that they yearly tranſported 10000 weight of its ore to *Rome*, to be prepared by themſelves, and employed in their paints and *fucus's*, was the ſame with that of *Almaden*, which I might confirm not only from the grain and red colour of this ore, which ſtill anſwers to *Pliny*'s deſcription, but from the ſituation of the place, which he mentions to be in the *Bœtica*, and the actual tradition of the country; but I chuſe rather to be producing obſervations, than entering upon diſcuſſions.

The firſt, which to me appears equally uſeful and ſimple, is their method of proving whether a ſtone contains mercury or not, and in what quantity. To this end they chuſe a ſtone, which by its weight and colour ſeems to have mercury in it; and heating a piece of it red-hot, when it appears covered with a bluiſh flame, withdraw it again, and place it under a kind of bell-glaſs, through which they obſerve the fumes that exhale from it; and if they condenſe and gather into little ſilvered drops which hang to the ſides of the glaſs, or drop down, they are ſure it contains mercury.

By this experiment I was led to another eaſier one for diſcovering any ſuſpected ſophiſtication in in a piece of cinnabar, which is by pulverizing it, and caſting the powder upon a burning coal, the colour of the flame will diſcover either the purity of the mineral, or the quality of the foreign body mixed with it; for if it be pure, the flame will appear thicker and of a blue colour, bordering

on violet, without almoſt any taſte; whereas if the flame border on red, it will be an indication that the cinnabar has been adulterated with red lead: if it exhibits a ſort of boiling, joined with an odorous fume, it is a proof of the admixture of *ſanguis draconis* therewith.

2dly, By obſerving the ſoil which the miners open to pull up the rock, even in thoſe places where the vein is richeſt, I could not perceive that they find thoſe quantities of running mercury which are commonly imagined; and if there ſometimes appear a few ounces, it is in conſequence of the violent ſtrokes which the miners give upon the rock with their iron inſtruments, or of the heat and exploſion of the gunpowder wherewith they ſometimes ſtorm the mines.

3dly, Enquiring whether there might not be minerals of ſome other ſpecies in theſe mines, according to the notion of ſome chymiſts, who hold mercury the principle of metals, I learned that no other kind had ever yet been obſerved, nor could I find any by my own ſearch.

My fourth obſervation relates to the manner of ſeparating mercury from cinnabar, which has ſomething in it very different from the *Spaniards* practice in *Peru*, and has ſcarce any thing in common with that uſed by the *Italians* in the mines of *Friuli*; for at *Guancavelica*, a celebrated quickſilver mine in *Peru*, the operation is performed in little furnaces, which obliges the workmen to cool the aludels by a quantity of water placed within-ſide thereof, as alſo by ſprinkling the outſide during the operation, in order the better to condenſe the mercurial vapours; whereas at *Almaden* it is the lengthening the line of theſe aludels, by continuing them from one end of the

terras

terras to the other, that does the office of refrigeration.

As to the method in the mines of *Friuli*, it is more operofe, yields lefs, and takes up more time and greater number of workmen than either of them, by reafon of the great number of lotions which the pulverized cinnabar undergoes in order to feparate the mercury therefrom, by its weight, 'ere they put the cinnabar, according to the method of that country, in retorts; whereas at *Almaden* three men, in the fpace of three days, and at a very moderate charge, will go through a coction which produces 3000 weight of mercury.

5thly, Another advantage in the operation of *Almaden*, is its fucceeding without any foreign addition, not even that of iron filings, which are commonly ufed every where elfe, in order to revivify the mercury without any lofs of its quantity, which the *Spaniards* attain at *Almaden* by the mixture of ftone and earth wherein the ore is inclofed, which ferve to retain or fetter the fulphurous parts of the mercury at an eafier expence than filings do in the retort.

Nor did it feem of lefs importance to obferve what effects the mercurial vapours might have both on the bodies of men employed in working the mines, and other bodies which happen to come within their atmofphere, and the common prejudice entertained againft thefe vapours, made me double my attention; but I learned in fine, that this opinion ought to be ranked among the number of popular errors, fince fo far is the earth over thefe mines from being barren, that on the contrary they are very fertile in grain and other fort of plants, which withal do not partake the leaft of any arfenical malignity fuppofed to be in the mercury wherewith the mountain abounds;

beſide that, the ſprings on the northern deſcent of the mountain afford water which the country people drink without any inconvenience.

The ſmoak itſelf, which in the time of the operation evaporates through the chimneys of the buildings oppoſite to the furnaces, and whoſe effect were more to be apprehended within the compaſs of the earth it ſpreads over, does not occaſion the leaſt alteration on the neighbouring trees, nor is at all felt by the inhabitants of the town who live neareſt the furnaces.

It is true, native cinnabar given inwardly, ſometimes produces effects very different from what were expected, as vomiting, gripes, *&c.* But theſe ſymptoms are only owing to the want of diſcretion in chuſing a cinnabar like that of the firſt vein above deſcribed, wherein is a great mixture of vitriolic parts in lieu of thoſe of the ſecond and third veins, which I have obſerved are more pure.

As to the accidents which frequently happen upon approaching the place where the miners are at work, I obſerve that they are often deceived in attributing them rather to a vapour ariſing from this particular kind of ore, than to that of any other place where no metallic ore is found, for going down the ſame winter into other ſubterraneous places, eſpecially the quarries of *St. Leu de Ceran* near *Chantilly*, which go very deep under ground, I was ſurprized at a good diſtance with a very ſour ſmell, which only aroſe from the ſweat of the men employed therein, and withal found a difficulty of reſpiration, as alſo pains in my limbs, much like thoſe wherewith I found myſelf ſeized in the mines of *Almaden*; but I was convinced at the ſame time, that thoſe different ſenſations were the neceſſary effects of too haſty

tranſitions

tranſitions uſual in ſuch places, from a hot air into a cold one, and from a dry to a moiſt one, ſince, as I have already obſerved towards the beginning of this memoir, there are ſome places in them ſo hot, that they ſerve as ſtoves to rheumatic people that come there to ſweat. Another common error touching the cauſe of the diſeaſes of thoſe who work in the mercury mines, is that they imagine it owing to the continual inſpiration of the vapours exhaling from it. This prejudice may be removed by comparing the ſtate of the miners at *Almaden*, who work voluntarily, with that of the ſlaves who are forced to it, the former by the care they take in returning from the mines to change all their working-cloathes from top to toe, and their ſhoes eſpecially, preſerve themſelves in good health, and attain to the ſame age as other men; whereas the poor ſlaves, being unable to ſhift their cloaths, and being even obliged to take their meals in the mines themſelves, where they handle their victuals without waſhing, are liable to tumours of the *parotides*, *apthæ*, ſalivations and puſtules all over heir whole body; all which are evidently the effect of the immediate contact, or rather admiſſion of the mercurial particles into the pores of the ſkin, as in the caſe of mercurial remedies.

The method uſed by the phyſicians of *Almaden* is very different from what we commonly practiſe on the like occaſion, which is to recur to purgatives and bleeding; the whole cure is effected by expoſing the patient to the open air, and adminiſtring ſome ſimple abſorbent, as burnt hartſhorn, ivory, crabs-eyes, or the like. And what is moſt remarkable in this cure is, that it ſucceeds almoſt univerſally in all ſober abſtemious perſons; whereas they who are given to drinking, die without

reſource,

resource. As to the slaves, *&c.* which at their coming into the mines were touched with any venereal disorder, they frequently find their cure therein.

It is nastiness therefore, with excess in drinking, and the continual contact of mercury, that in a length of years occasion those tremors wherewith the workmen are seized; which however are not constant, but grow more or less sensible, as they are more or less seized, with the motions of fear or surprize, the sad effects of a detention of the blood in the vessels of the brain, when rendered varicous by the weight of certain particles of mercury lodged therein, which equally befalls those who have taken mercury unseasonably, or in too great quantity.

An explanation of the figures, which belong to the mines of Almaden, *translated by* J. M.

Plate X. *Fig.* 1.

A view of the park, in which are the furnaces, where the quicksilver is made, and the royal magazine where it is kept.

a a a a, the wall of the park.

b b b b b, *&c.* furnaces in which the quicksilver is separated.

c c c, sheds under which they put the wood and other instruments serving for the furnaces.

d, the royal magazine, in which the quicksilver is kept under several keys.

Fig. 2. gives a large representation of the furnaces serving for the separation of the quicksilver.

A, two furnaces joined together, in which are disposed the pieces of mineral cinnabar, which are to furnish the quicksilver.

B,

B, the door of one of the furnaces, open on the ſide, even with the grate, and ſerving for a paſſage to place the pieces of mineral upon the grate.

C C, the domes of the two furnaces.

D D, the tubes of the chimnies of the two furnaces.

E E E, ſixteen apertures in form of ſpiracles, in which the aludels are inoſculated.

F F F, the terraſs which ſerves for communication of the furnaces with the chambers, which ſerve for a receiver.

G, the ſtairs which lead to the terras.

H H, the line of aludels.

I, heaps of aludels, of which other lines are to be formed.

K, a building divided into four chambers, in which the ſmoak is collected, which is carried thither by the aludels.

L L L, ſixteen ſpiracles or apertures, in which the aludels are inoſculated, and anſwer to thoſe of the furnaces.

M M M M, the windows, by means of which they enter into the chambers.

N N N N, tubes of chimnies; by which the ſmoak is conducted into theſe chambers by the aludels.

Fig. 3. a plank *in equilibrio*, ſerving as a balance to weigh the heavy parts of the ore.

Fig. 4. lumps of ore.

Fig. 5. the hammer and chizzel uſed to get out the ore.

Fig. 6. The carriage, which ſerves in the mine to carry the earth and fragments of cinnabar.

Fig. 7. planks to be laid on the carriage to ſuſtain the *cabats* or baſkets.

Fig.

Fig. 8. a ſcuttle, to carry the iron inſtruments uſed in breaking the ore.

Fig. 9. a bag made of ſkin, filled with quickſilver, and ready to be ſent into the *Indies.*

Fig. 10. an aludel.

Fig. 11. a great earthen pot, in which the quickſilver is put, to be kept in the royal magazine.

Fig. 12. a baſket, which they fill with little fragments of the ore, and lay upon the little carriage.

Fig. 13. a ſort of mould, to give a form to the clods compoſed of fat earth and ſmall fragments of ore.

IV. *A compariſon of ſome obſervations made by the Chevalier* de Louville, *with thoſe which have been made at the obſervatory, by M.* Maraldi.

As M. *de Louville* ſees the ſteeple of *St. Croix* of *Orleans*, from the place of his obſervations, which is called *Carré*, he has determined in toiſes the diſtance between theſe two places, by a meaſured baſe, and by the two angles obſerved at the extremity of the baſe. By this diſtance and the angle of poſition, he has found the parallel diſtance between his houſe and *St. Croix* to be 1765 toiſes, which make 2′ 45″ of a degree difference of the meridians, by which the houſe is more eaſtward than *St. Croix*; he has found alſo by the ſame operation the meridian diſtance of 233 toiſes which make 15′, by which the houſe is more northward. Thus the poſition of his houſe is determined with regard to *St. Croix.*

The

A View of the park in which are the furnaces wherein the seperation of the quick silver is made at Almaden

J. Mynde sc.

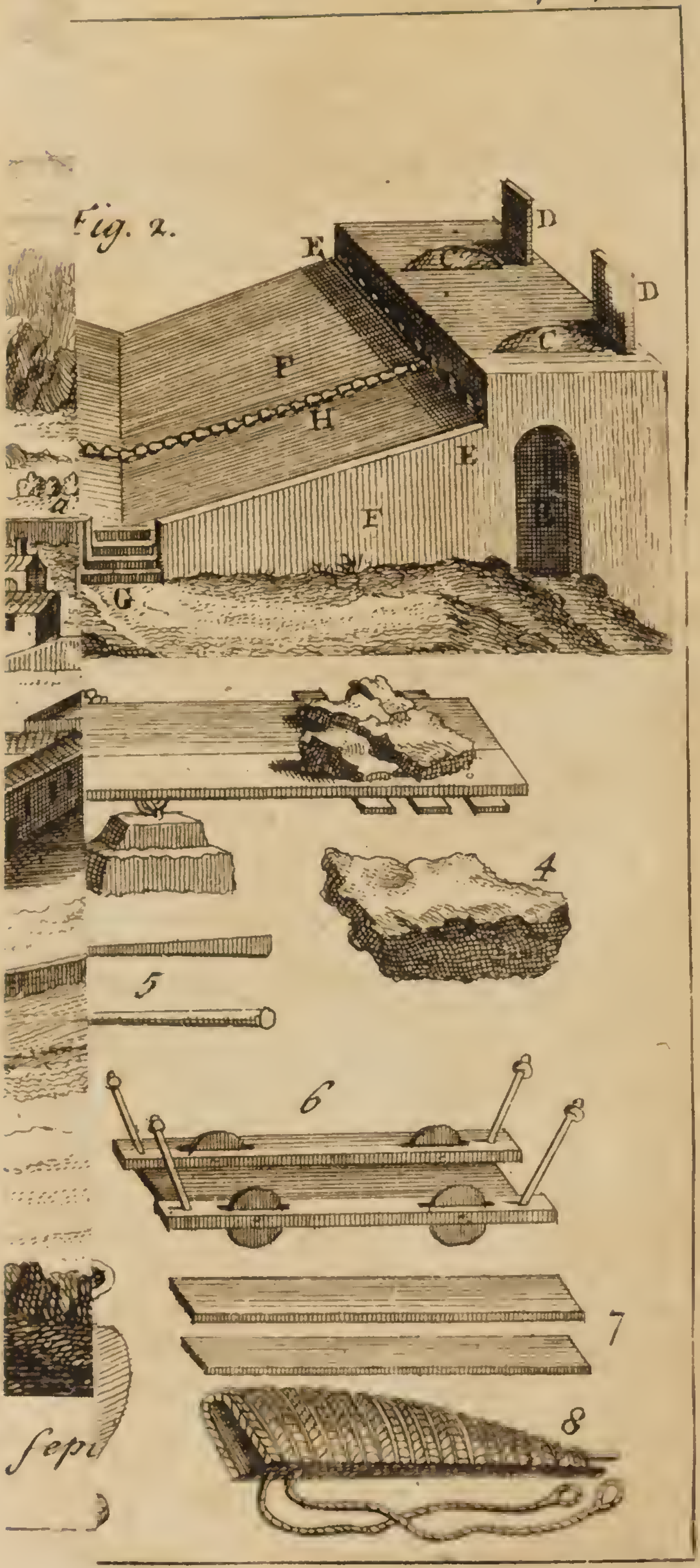
Fig. 2.
D
D
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The ſituation of *St. Croix*, with regard to the obſervatory, was determined by geometrical operations in the firſt meridian journey of the late M. *Caſſini*. For the meridian diſtance between the obſervatory and *St. Croix*, was found to be 53,319 toiſes, which give 56′ 2″ of a degree. And ſuppoſing the height of the pole of the obſervatory 48° 50′ 10″, the height of the pole of *St. Croix* will be 47° 54′ 8″, and of M. *de Louville*'s houſe 47° 54 23″.

The parallel diſtance between the meridian of the obſervatory and *St. Croix*, was determined in the ſame journey to be 16,396 toiſes, which make 25′ 36″ difference of longitude, by which *St.Croix* is to the weſt, but M. *de Louville*'s houſe, by the operations which he has made, is 2′ 45″ more eaſtward than *St. Croix*, therefore the difference of the longitudes between the houſe and the obſervatory amounts to 22′ 51″, which make 1 31″ of time, by which the houſe is more weſtward. This is the difference which reſults from the geometrical method; and that alſo has been tried which depends on celeſtial obſervations.

M. *Caſſini* and I obſerved the emerſion of the firſt ſatellite from the ſhadow of *Jupiter*, which was obſerved alſo by M. *de Louville*. M. *Caſſini* obſerved it in the lower apartment, with a teleſcope of 16 feet, at 10h 5′ 8″. I obſerved it in the upper apartment, with a teleſcope of 17 feet at 10h 5′ 7″, ſo that we agree within about a ſecond in this obſervation, which happens pretty often in like obſervations. M. *de Louville* has obſerved with a teleſcope of 23 feet the ſame emerſion at *Carré*, near *Orleans*, at 10h 3′ 29″. Before I compare this obſervation with our own, we muſt obſerve, that the teleſcope which M. *de Louville* made uſe of, is 6 or 7 feet longer than thoſe which

we uſed, thus with his teleſcope he muſt have ſeen the emerſion ſooner than he would have ſeen it with a teleſcope of the length of ours, and by ſeveral experiments made for a long time, we have found, that with a teleſcope of 23 feet we ſee the emerſions 7 or 8 ſeconds of time ſooner than with a teleſcope of 17 feet. We muſt therefore add theſe 7 or 8 ſeconds to $10^h\ 3'\ 29''$, the hour of M. *de Louville*'s obſervation, and we ſhall have $10^h\ 3'\ 37''$, which being ſubſtracted from $10^h\ 5'\ 8''$, the time of our obſervation, we ſhall have the difference of the meridians $1'\ 31''$ or $32''$ of time, as it reſults by the other method, which we found to be $1'\ 31''$.

This experiment is not the only one that we have of the wonderful agreement between theſe two methods in the determination of the difference of the meridians. We alſo found laſt year the ſame exactneſs at *Dunkirk*, at the moſt northern and ſouthern extremity of the meridian of the obſervatory, and in 1701, toward the ſouthern extremity. The wonderful agreement of theſe two methods, ſo different in ſo many points, ſhew the exactneſs of both of them.

An

An explanation of the Terms *of* Art *uſed in this Volume, which were not explained at the End of the former Volumes.*

A.

ANnuli *of an inſect are thoſe rings, which divide the body as it were into ſeveral partitions.*

C.

Caliper is a peice of board notched triangularwiſe, in the middle, for the taking of meaſures. It is uſed by carpenters and joiners.

M.

Mark, a weight uſed for gold and ſilver. The *French* mark weighs 8 of their ounces, or 4608 of their grains.

T.

Trunnion of a mortar, or of a cannon, is the knob of metal, which bears it up on the cheeks of the carriage.

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